RICE UNIVERSITY

Essays on Banking and Debt Contracting

by

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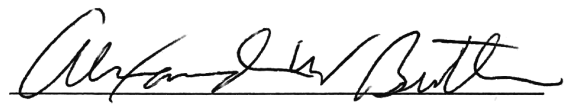
A Thesis Submitted
in Partial Fulfillment of the
Requirements for the Degree

Doctor of Philosophy

Approved, Thesis Committee:



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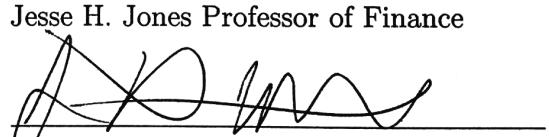

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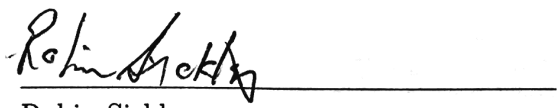

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ABSTRACT

Essays on Banking and Debt Contracting

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This dissertation contains three chapters. In the first chapter, I investigate whether restrictive loan covenants disrupt or improve firm operating performance. Using an instrumental variables approach to address the endogenous relationship between covenant strictness and firms' efficiency, I find stricter loan covenants cause an increase in profitability and a reduction in operating cost. Stricter covenants improve performance only in firms with poor governance: those without large shareholder ownership, with weaker shareholder rights, facing softer competition in their product market, or with inside director dominated boards. The evidence is consistent with the view that the design of debt contracts can mitigate agency costs in firms that lack alternative governance mechanisms.

A small number of interconnected banks increasingly dominate the syndicated loan market, one of the largest sources of external finance for firms. In the second chapter, I use measures from network analysis to test whether lender interconnectedness benefits borrowers through efficiency gains, or is harmful due to increased market power. Traditional measures of industry concentration are not relevant in the syndicated loan market because lenders share information and resources. Using bank mergers to identify exogenous variation in lenders' interconnectedness, I find that an increase in lender network centrality reduces firms' cost of borrowing. The effects are larger

for borrowers with higher levels of information asymmetry, consistent with theories suggesting that facilitating information sharing increases lending efficiency.

The third chapter examines whether creditor protection rights affect the structure of corporate debt. Improving legal protection of creditor interests may expand firms' access to debt markets, but also increases creditors' rights to intervene with firm policies. Using variation from legal rulings in Delaware, I find that increasing creditor protection leads to a reduction in senior secured debt and an increase in unsecured bonds, especially for firms close to bankruptcy. Reducing creditors' ability to sue managers for breach of fiduciary duty leads to a large increase in secured debt with more restrictive covenants. The results suggest that legal protection of creditor interests affects firms' choice of debt structure.

Dedications

To my father, who showed me what hard work means. Everything I have achieved I owe it to you. Rest in peace.

To my mother, who taught me perseverance.

And to my lovely wife, my best friend who makes me want to become better every day.

Acknowledgments

I am deeply indebted to my mentor and dissertation chair Alex Butler for his invaluable support, guidance, and patience. I am honored to have completed my PhD dissertation under his supervision.

I am extremely thankful to Gustavo Grullon, James Weston, and Margarita Tsoutsoura. I greatly appreciate their constant encouragement and the time they spent advising me to improve my work; I am also grateful to the Jones School family: the students, the faculty, and the staff for providing support. Finally, I am thankful to the Economics department and Rice University for providing an amazing workplace.

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Tough Love: The Causal Effects of Debt Covenants on Firm Performance

1.1 Introduction

Do loans with restrictive covenants disrupt or improve borrowers' operating performance? The effect of loan covenants on firm efficiency is an important economic question because bank loans are one of the most important sources of external finance for firms. Estimating the causal effect of restrictive debt covenants on firm performance is a challenging task, because the design of a loan contract is endogenous to firm characteristics (Jensen and Meckling (1976)). Using an instrumental variables (IV) approach to address this identification issue, the main contribution of this paper is to show restrictive loan covenants have a causal impact on borrowers' profitability and cost efficiency.

Theory suggests restrictive debt covenants may have both a positive and a negative effect on firms' operating performance. On the one hand, covenants can have a negative effect on profitability by constraining managers' flexibility to make optimal decisions, and by altering the choice of projects and financial policies (Jensen and Meckling (1976), Tirole (2010)). On the other hand, restrictive loan covenants can also have a positive effect on firm performance by disciplining the manager (Myers (1977), Dewatripont and Tirole (1994)). The optimal set of covenants therefore

should balance the conflicts of interest between creditors and shareholders in a way that maximizes the value of the firm (Smith and Warner (1979)). Yet, due to the separation of ownership and control, self-serving managers have an incentive to set covenants looser than what is optimal for the firm in order to protect their jobs, maintain their flexibility, and maximize their personal utility (see Donaldson (1963), Myers (1977)). Therefore, when intense agency conflicts occur between managers and shareholders, creditors can promote corporate efficiency by putting the business under a shorter leash with more restrictive debt covenants (Baird and Rasmussen (2006)).

The main challenge in estimating the causal effect of stricter debt covenants on firm performance is finding variation in covenant strictness that is exogenous to firm characteristics. Poorly performing firms are more likely to get stricter debt covenants (see Aghion and Bolton (1992), Nini et al. (2009), and Roberts and Sufi (2009a)), or firms may choose tighter covenants to signal positive private information (see Demiroglu and James (2010) and Beyhaghi et al. (2016)). I address this endogeneity issue using an IV approach.

A valid instrument for covenant strictness needs to affect the propensity of banks to impose stricter covenants, but it must be unrelated to borrower characteristics. A variable that satisfies both conditions is the number of defaults in the portfolio of the lead lender in the six-month period prior to the origination of the loan. Consistent with the findings of Murfin (2012), I find that when banks experience defaults in their loan portfolios they impose more restrictive debt covenants on new loans. Importantly, the change in covenant strictness is driven by shocks that affect

only the lead arranger (the supply side) and is plausibly exogenous to unobserved borrower characteristics. To make the exclusion restriction more plausible, I mitigate the possibility that unobservable industry- or location-specific factors affect both borrowers' future performance and defaults in the lead arranger's portfolio (see Hertzel and Officer (2012)). To this end, I exclude from the default-count measure the defaulting firms that come from the same industry or state as the firm that receives the new loan.[1] The identifying assumption is that the defaults of firms located in different states and different industries affect the performance of a firm only through the strictness of the debt contract with the lead arranger.

I find that stricter debt covenants have a positive and economically large effect on profitability (ROA). In particular, a one standard deviation exogenous increase in covenant strictness causes a 2.6% increase in profitability one year after the start of the loan. The effect is economically larger in the subsample of borrowers that have a past credit relationship with the lead arranger, because these firms are less likely to engage in loan shopping due to high information costs (see Sharpe (1990) and Rajan (1992)). Stricter covenants also lead to a reduction in firms' operating costs, which suggests firms operate at a lower unit cost and increase their profit

[1]Consider, for example, the loan Bank of America made to Gap, Inc. on June 25, 2003. To construct the IV, I count how many active loans in the portfolio of Bank of America defaulted in the 180-day period leading up to Gap's loan, that is, 12/25/2002-06/25/2003. In this default-count variable, I do not include defaults of firms from the same state or industry as Gap. For example, two of the companies that defaulted six months prior to the loan Bank of America made to Gap are United Airlines (headquartered in Illinois) and Weirton Steel (headquartered in West Virginia). Therefore, the IV will not satisfy the exclusion requirement if we believe the defaults of a steel company from West Virginia (Weirton Steel) and an airline company from Illinois (United Airlines) correlate with unobserved factors that determine the operating performance of an apparel company in California (Gap).

margins. Although I do not find evidence that covenants affect employment, wages, or advertising expenses, there is weak evidence suggesting that firms reduce R&D expenses and innovation.

I investigate whether the positive effect of covenant strictness on firm performance is due to managerial myopia. For instance, in response to stricter covenants, managers might try to boost short-run profitability at the expense of long-run performance, they might try to manipulate earnings, or choose riskier projects. The evidence is not consistent with this conjecture. Stricter covenants do not lead to a reversal in long-run profitability, and do not increase earnings management or operational risk.

The positive effects of debt covenants on profitability raise the following question: if stricter covenants improve firm performance, why do firms not always adopt stricter terms? Agency conflicts. Due to the separation of ownership and control, managers could choose loan terms that maximize their personal utility instead of shareholders'. For instance, managers prefer looser covenants to reduce the possibility of a covenant violation that would allow creditors to take control of the management (see Roberts and Sufi (2009b), Nini et al. (2012)). Therefore, managers, who make decisions on behalf of shareholders, might exploit their position to the detriment of firms' operating efficiency, especially when the firm does not have strong governance mechanisms in place to mitigate these agency conflicts.

I hypothesize that if financial covenants mitigate managerial agency costs and have a disciplinary effect that forces managers to improve operating efficiency (Harris and Raviv (1990)), stricter covenants should have a positive effect only on firms that lack alternative governance mechanisms to monitor and discipline the manager. To

test this hypothesis, I investigate whether stricter covenants have a larger effect on profitability for firms with larger manager-shareholder conflicts. Specifically, agency conflicts are greater and managers are less likely to maximize operating efficiency if firms face softer competition in their product market, if they lack large monitoring shareholders (blockholders), if inside directors dominate the board, or if managers are entrenched and protected from takeovers.[2] I find that an exogenous increase in covenant strictness increases operating efficiency only in firms with higher agency conflicts between managers and shareholders. Therefore, the heterogeneous impact of debt covenants on performance suggests that the design of debt contract terms can substitute for alternative external or internal governance mechanisms.

This paper builds on previous work that investigates how firm characteristics determine the choice of loan covenants. For example, Demiroglu and James (2010) show that firms' expected performance affects the selection of loan covenants. Nini et al. (2009) find that under-performing firms are more likely to receive covenants restricting investment. These papers show that firm performance is an important determinant of covenant selection, and highlight why it is difficult to estimate the causal effects of loan covenants without variation that is independent of firm characteristics.This paper contributes to the literature by offering empirical evidence consistent with theory that suggests that financial covenants have a causal effect

[2]Without being exhaustive, the following work supports the view that poor governance leads to poor firm performance: Shleifer and Vishny (1986, 1997), Bertrand and Mullainathan (2003), Gompers et al. (2003), Cremers and Nair (2005), Bebchuk and Cohen (2005), Chen et al. (2007), Edmans (2009), Giroud and Mueller (2010, 2011), and Cohen and Wang (2013).

on firms by controlling agency problems (Myers (1977), Smith and Warner (1979), Berlin and Mester (1992)).

This paper is also related to studies that investigate the effects of covenant violations on firm outcomes, such as financial policy (Chava and Roberts (2008), Roberts et al. (2009)), performance (Nini et al. (2012)), unemployment (Falato and Liang (2016)), and establishment closures (Ersahin et al. (2016)). Covenant violations allow creditors to intervene with firm policies directly, so these studies estimate the effects of ex post creditor intervention. This paper tests a different hypothesis. Can creditors affect firm performance without intervention by making more restrictive covenants ex ante? This hypothesis is different because it suggests that imposing strict loan covenants at origination has a causal impact on firm performance through managerial incentives. By exploiting an exogenous source of variation in covenant strictness, the empirical evidence in this paper is consistent with theory that suggests that the design of loan contracts plays an important role in aligning incentives between managers and shareholders (Dewatripont and Tirole (1994)).

The paper proceeds as follows. Section I describes the data and details of the empirical methodology. Section II presents the baseline results, and section III discusses the governance role of debt. In section IV, I test the robustness of the results, and section V concludes.

1.2 Data and Empirical Strategy

1.2.1 Data and Summary Statistics

My empirical analysis focuses on primary issues of syndicated loans, one of the largest sources of external finance for firms. Loan Pricing Corporation's (LPC) Dealscan database contains borrower-lender information of bank loan agreements, including financial covenants and performance-related restrictions. According to LPC's product manager, Dealscan covers 98% or more of global syndicated loans but varies in years.[3] I merge loan, lender, financial covenant, and pricing data from Dealscan with quarterly accounting information from Compustat using the link by Chava and Roberts (2008).

The merged Dealscan-Compustat sample consists of relatively large firms, because large firms have access to the market for syndicated loans. I exclude financial firms because their profitability and investment profiles significantly differ from non-financial firms. I do not exclude utility firms from the analysis, but the results do not change when I drop these observations. I also exclude loans whose primary purpose is to finance leveraged buyouts, because after the buyout, these firms usually go through severe restructuring and are likely to experience cash flow increases even without changes in efficiency (Hillier et al. (2011)).

My final dataset consists of 3,701 loan packages. A loan package can contain multiple loans, or facilities, of different types (e.g., term loans, revolvers, bridge loans), and each facility may have different characteristics, such as different maturity,

[3]This statement comes from the online presentation of Dealscan in WRDS from LPC's product manager: `http://wrds-web.wharton.upenn.edu/wrds/E-Learning/_000Video/Overview_of_Dealscan/index.cfm`.

amount, and spread. However, covenants pertain to the whole loan package and are the same for all loan facilities in this package. To avoid duplicating covenant and accounting information, therefore, the observations in my sample are at the package level.

For every loan in the data, I identify the name of the borrower, the lender, the primary type and purpose of the loan, as well as its maturity and amount. I follow Chava and Roberts (2008) and define loan maturity as the number of months between the earliest loan origination date and the latest maturity date in the loan package. To define loan amount, I use the sum of all the amounts in a loan package.

The Appendix provides definitions for all variables, and Table 1.1 provides summary statistics for my sample. I winsorize the data at the 1st and 99th percentile to mitigate the effects of outliers. An average firm has approximately $3.3 billion in assets, and annual profitability (EBITDA/Assets) of 15%. The maturity and amount of an average loan package is four years and $475 million, respectively. Approximately 47% of the loans are secured (backed by collateral), and 9.7 banks participate in the syndication. The number of participating banks is an important control for contract strictness, because a large number of participants can potentially reduce the incentive of lead arrangers to screen and monitor the syndicated loan (see Gopalan et al. (2011) and Sufi (2007)). The summary statistics are consistent with the averages reported in previous studies using Dealscan as a primary source of loan information (see Ivashina (2009)), Roberts and Sufi (2009b), Murfin (2012)).

The main dependent variable and measure of profitability that I use in this paper is ROA measured as EBITDA/Assets, calculated on a rolling four-quarter basis. I use

EBITDA instead of net income because EBITDA captures operating cash flows more directly than net income, and excludes the effects of capital structure or taxation. I decompose EBITDA/Assets into two parts, EBITDA/Sales and Sales/Assets, and study how covenant strictness affects these measures of efficiency. This identity, known as DuPont identity, is informative because it explains whether operating efficiency (EBITDA/Sales) or asset use efficiency (Sales/Assets) drives the change in profitability. I also use two measures of cost efficiency: cost of goods sold divided by sales, and administrative expenses divided by sales.

I have excluded from the regressions all firms that violate a financial covenant at loan origination and within a year from origination.[4] Covenant violations are technical defaults that allow creditors to accelerate loan payments and pave the way for creditors to directly intervene with firm policies. I exclude from the sample firms that violate a covenant within the first three quarters to mitigate the possibility that firm outcomes are directly driven by creditor intervention.[5] I also exclude from the analysis facilities after renegotiations or covenant violations that appear as new loans and whose origination date starts before the expiration of the previous loan.

With accounting and loan data in place, I construct the two main variables of interest: covenant strictness and the number of defaults the lead arrangers have suffered six months prior to making the loan. I construct these variables along the

[4]Amir Sufi generously provides the data on covenant violations on his website.

[5]I am particularly thankful to Cem Demiroglu for this suggestion. Nini et al. (2012) find that firm profitability increases after covenant violations, and therefore I expect that excluding loan violations to work against the principal findings of this paper.

lines of Murfin (2012), and give a brief description of the process and summary statistics in the next two subsections.

Loan Covenant Strictness

To measure the strictness of loan covenants, I use a new measure from Murfin (2012), based on the probability of a covenant violation. Unlike a covenant-intensity index that simply counts the number of covenants in a debt contract, this measure of covenant strictness combines the total number of covenants, how tightly they are set, and also adjusts for the variance-covariance of the accounting ratios on which these covenants are based. As a result, this measure of covenant strictness provides a superior measure compared to other measures researchers have used in the literature.[6] For example, Hertzel and Officer (2012), Kjenstad et al. (2013), and Bradley and Roberts (2015) use a covenant-intensity index, which simply counts the number of covenants in the loan contract. Counting the number of covenants (covenant intensity) does not necessarily imply covenants are stricter, especially if these restrictions are boilerplate covenants the firm will most likely not violate. For example, consider two firms with a financial covenant that requires both firms to keep their Debt/EBITDA ratio below 3. If, at the time of contracting, firm A's EBITDA/Debt ratio is 1, but

[6]Demerjian and Owens (2014) hand-collect 5,278 covenants from a sample of 2,100 loans of the original Tearsheets in Dealscan and find that the definitions of the financial ratios on which covenants are defined are not homogeneous. They find, for example, 356 different definitions for Fixed Charge Coverage covenants in Dealscan. In this case, constructing this ratio using Compustat data could potentially contain measurement error. To address this issue, I construct the accounting ratios based on the definitions of the most popular and homogenous covenants according to Demerjian and Owens. However, the authors conclude measurement error is not likely to significantly affect the results, and argue in favor of a comprehensive measure of contract strictness such as the one Murfin (2012) uses.

is 2.8 for firm B, then simply counting the number of covenants cannot capture the fact that firm B is more likely to violate the covenant. Demiroglu and James (2010) create a measure of covenant tightness that captures this effect, but this measure does not account for the total number of covenants in the debt contract.

The measure of covenant strictness that I use in this paper proxies for the probability that the firm will violate at least one covenant over the next quarter. The Appendix describes how to estimate covenant strictness following Murfin's method. In Table 1.1, the median of loan covenant strictness, or the probability of violating at least one covenant, is 18.3%. Creditors intentionally impose tight covenants at the time of origination, and loosen them later through renegotiations as information asymmetry between lenders and borrowers declines (see Garleanu and Zwiebel (2009)). Roberts et al. (2009) show that over 90% of long-term debt covenants are renegotiated prior to their stated maturity, not necessarily due to financial distress, but because the terms of the initial covenant are particularly strict. This evidence is similar to Denis and Wang (2014), who find evidence that creditors exert strong control rights over their borrowers' operating and financial policies in a state-contingent manner through covenant renegotiations.

Instrumental Variable: Lenders' Defaults

The instrument for covenant strictness that I use in this paper is the number of defaults lead arrangers have suffered to their loan portfolios in the six-month period before issuing a new loan. I count the number of defaults from firms whose S&P long-term debt rating switched to default or selective default during the period

that the firm had an outstanding loan with the bank. The purpose of the default count measure is to extract variation in covenant strictness from factors that are idiosyndcratic to the creditor and affect the supply side of strictness, and at the same time are unrelated to firm characteristics and do not affect the demand side of covenant strictness.

Dealscan provides information on all lenders participating in a syndicated loan. I only count the number of defaulting firms in the portfolio of lead arrangers and not of participating banks. Gopalan et al. (2011) show that failing to screen or monitor borrowers that perform poorly has a detrimental effect on the reputation of lead arrangers, and makes attracting participants in future syndications harder. Lead lenders are therefore more likely to change their monitoring in response to a larger number of defaults in order to improve their reputation as loan underwriters.

Lead arrangers have an average of 1.3 (median is zero) defaults in their loan portfolio six months before making a new loan. In constructing the default-count measure, I exclude defaults of firms that have the same one-digit SIC code or are headquartered in the same state as the new borrower. This procedure mitigates the concern that industry- or location-specific risk drives the change in strictness, and, therefore, makes it less likely that industry contagion affects covenants strictness (see Hertzel and Officer (2012)). On the other hand, if changes in covenant strictness come from lender-idiosyncratic shocks, such as the number of defaults in a lender's portfolio, then one can more easily argue that contract changes come from the supply side and are therefore exogenous to firm performance.

1.2.2 Empirical Strategy

The most straightforward method to estimate the impact of covenant strictness on firm efficiency is via an OLS regression. In subsection B.1, I explain why estimates of OLS regressions could be biased downward. In subsection B.2, I describe the IV/2SLS methodology that I use in this paper to get unbiased estimates for the impact of covenant strictness on firm efficiency.

Simultaneity

The purpose of this section is to illustrate why the OLS estimates of covenant strictness on firm efficiency are biased downward in the presence of simultaneity. To understand the effect of simultaneity in OLS estimates, consider the following simple system of equations, where y is firm efficiency and x is covenant strictness:

$$y_{t+1} = \beta x_t + u_{t+1} \tag{1.1}$$

$$x_t = \alpha y_{t+1} + v_t. \tag{1.2}$$

The coefficient β represents the effect of covenant strictness on firm efficiency. To get an unbiased and consistent estimate of $\hat{\beta} = \beta + Cov(x_t, u_{t+1})/Var(x_t)$, we need the exogeneity condition $Cov(x_t, u_{t+1}) = 0$. In a system of equations as in (1) and (2), the absence of the exogeneity condition, the bias is given from the formula shown

below and depends on the signs and relative magnitudes of the coefficients α and β.[7]

$$\frac{Cov(x,u)}{Var(x)} = \frac{\alpha(1-\alpha\beta)Var(u)}{\alpha^2 Var(u) + Var(v)} \tag{1.3}$$

Assume stricter covenants have a positive impact on firm efficiency, so the true β is positive. Also, the existing literature suggests more profitable firms tend to get looser covenants (see, Nini et al. (2009), Rauh and Sufi (2010)), and therefore α is negative. Then from equation (3), it becomes evident that the bias term is negative and the estimated coefficient $\hat{\beta}$ of covenant strictness is negatively biased.

IV/2SLS Regressions

To obtain the IV/2SLS estimates, I start by estimating the following first-stage regression similar to Murfin (2012):

$$Strictness_{i,t} = \alpha_j + \alpha_t + X'_{i,t}b + \gamma Defaults_{i,t-} + \epsilon_{i,t}. \tag{1.4}$$

In this regression, subscript i refers to the firm, j refers to the three-digit SIC code of the firm, t refers to the loan's origination year, and t- refers to the six month period before the loan issuance. If γ is positive one default to the lead arranger's loan portfolio six months prior to this loan increases covenant strictness by γ, or in other words, increases the probability that the borrower will violate at least one covenant over the next quarter by γ.

[7]For more details on how the bias of the estimator could be positive or negative see Roberts and Whited (2012).

In the second-stage regression, I exploit the variation in covenant strictness coming from the number of defaults to lenders' loan portfolios and estimate the effect of covenant strictness on future efficiency. The second-stage regression therefore is

$$\Delta(ROA)_{i,t+1} = \alpha_j + \alpha_t + X'_{i,t}b + \delta\widehat{Strictness}_{i,t} + \eta_{i,t}. \tag{1.5}$$

The dependent variable in the second-stage regression is the change in firms' profitability one year after the origination date of the loan. The coefficient δ next to the predicted strictness is the estimate for the change in firms' operating efficiency due to changes in covenant strictness. The main measure of efficiency that I examine is profitability (ROA), but I also use the same empirical methodology for the other profit and cost-efficiency measures. Along with the point estimates of covenant strictness, I use the Anderson and Rubin (AR) Wald test to test the null hypothesis that the instrument is not relevant and weak.

1.3 Empirical Results

In this section, I present the empirical evidence from estimating the causal effect of covenant strictness on operating efficiency. The main variable of interest is the change in profitability (ROA) one year after the loan. In the first part of this section, I verify the validity of the IV by regressing debt covenant strictness on the number of defaults on the portfolio of the lead arranger 180 days prior to the origination of the loan. In the second part, I present the results from IV/2SLS regressions of profitability on debt covenant strictness.

1.3.1 First Stage: The Effect of Lender Defaults on Covenant Strictness

To extract variation in covenant strictness that is exogenous to firm characteristics, I first regress covenant strictness on the number of recent defaults in the loan portfolio of the lead arranger as in equation (4). A positive and significant γ in this regression indicates an increase in the number of defaults in the portfolio of the lead arranger shortly before making a new loan leads to an increase in covenant strictness of new loans. To ensure lenders change the strictness of debt covenants independently of firm characteristics, I construct the default-count variable so that it includes only defaults coming from firms in different industries and different states from the current borrower. This procedure does not significantly affect the strength of the instrument (relatedness), but makes the exclusion restriction more plausible.

Table 1.2 shows the results of estimating equation (4) following the methodology of Murfin (2012). This step is important in proving the instrument is relevant. The baseline regression in the first column has no fixed effects and includes controls only for firm characteristics, such as debt/assets (control for leverage), fixed-charge coverage (cash flow), current ratio (liquidity), and total assets (size), loan characteristics such as loan maturity, the number of participating lenders, the amount of the loan, and the loan spread. The coefficient of lender defaults is positive and statistically significant, which implies shocks in the portfolio of the lead lender affect the strictness of covenants in new loans. The economic magnitude of the estimated coefficient in the second column suggests a one-standard-deviation increase in lenders' defaults

(=2.2) increases covenant strictness approximately by 1.8% (=2.2*0.82), or in other words, the median firm is 1.8% more likely to violate a covenant in the next quarter.[8]

The results in Table 1.2 suggest the IV satisfies relatedness. In the first-column regression of Table 1.2, using no loan or lender characteristics or fixed effects, the F-statistic is significant at the 1% level. To isolate other macroeconomic factors, or industry and firm characteristics that could affect covenant strictness, I use year, industry, rating, loan purpose, and lender fixed effects in the middle column. The role of fixed effects in the regressions is to absorb the variation in covenant strictness explained within a certain year, within a certain lender, a certain industry, rating group, and loan purpose. The introduction of these fixed effects does not significantly change the magnitude or the statistical significance of the effects of lenders' defaults in covenant strictness.

Lenders may change their risk profile after experiencing defaults in their portfolio, and may shift toward safer firms. If lenders indeed switch to safer or more profitable firms after suffering more defaults, the effect of defaults on lenders portfolios should yield the opposite effect on covenant strictness, because safer borrowers can get loans with looser covenants, and therefore defaults would lead to either no effect or looser covenants. To address the possibility that unobserved firm characteristics drive the results, the last column on Table 1.2 presents the first-stage-regression results using firm fixed effects. In this regression, the coefficients are driven from variation of

[8] A 1.8% increase in strictness means that over a one-year horizon, the probability that the median firm will violate at least one covenant rises from 54.7% to 59.2%. The probability of violating a covenant one year after the beginning of the loan is calculated using the following formula: $\sum_{t \leq T} p(1-p)^{t-1}$, where p stands for covenant strictness, or the probability of violating a covenant in the next quarter.

strictness within the firm, and suggest that a larger-than-average number of defaults in the portfolio of the lender leads to an increase in the strictness of this firm's debt covenants. The panel dataset is unbalanced, and more than half the firms in the sample have at most two loans. As a result, the time-series variation within firms is not sufficient to exploit using firm fixed effects. For the remainder of my empirical analysis, therefore, I use industry (SIC-3) fixed effects instead of firm fixed effects, even though with firm fixed effects, the economic magnitudes of the second-stage regressions increase. Finally, using lender fixed effects, I also control for the effects of unobserved lender characteristics on debt contracting.[9]

Financial Crisis Subsample

The regressions in Table 1.2 do not include loans originated during the financial crisis of 2007-2009. During the crisis, a greater number of corporate defaults in the portfolio of a lead lender might not necessarily reflect the lender's inability to appropriately screen and monitor its borrowers, but could be the outcome of an ailing economy. Therefore, even if lenders do not experience a larger than average number of defaults in their portfolio, they might still impose stricter covenants due to an overall tightening of credit standards during the financial crisis.[10] As a result, the number of defaults in the lead lender's portfolio (the IV) does not have a statistically significant

[9] Another common method researchers use in the literature is demeaning the dependent variable (default count) by subtracting the average default count by lender. Gormley and Matsa (2014) assert that demeaning the dependent variable produces inconsistent estimates, and recommend using fixed effects instead. For this reason, I use lender fixed effects for all regressions.

[10] See the Federal Reserve survey of senior loan officers (Board of Governors of the Federal Reserve System)

effect on debt covenant strictness (regression results not tabulated). Therefore, to ensure that the IV remains valid throughout the sample period, the empirical analysis in this paper excludes the financial crisis.

1.3.2 The Causal Effect of Covenant Strictness on Firm Performance

With a valid instrument for covenant strictness, I regress the change in profitability one year after the origination of the loan on the first-stage estimates of covenant strictness as in equation (5). The first three columns of Table 1.3 present the OLS regression results using the endogenous IV of interest, covenant strictness. The last three columns present the IV/2SLS regression results after using an instrument for covenant strictness. In all regressions, I use industry (SIC-3), lender, year, loan purpose, and rating fixed effects. In columns 3 and 6, I replace industry fixed effects with more restrictive borrower fixed effects. I cluster standard errors at both the firm and lender level. According to Petersen (2009), clustering at the firm level addresses the bias in the standard errors due to time-series correlation within firms, and clustering at the lender level addresses the bias from correlation within lenders. To control for mean reversion and non-linearity in the regressions, I also include lagged profitability and its squared value.

The results from the OLS regressions in the first three columns of Table 1.3 indicate stricter covenants do not affect future profitability. The regression in the first column uses the full sample, and the results suggest covenant strictness has a negative and statistically insignificant effect on profitability. However, this empirical strategy causes difficulty in distinguishing whether restrictive covenants have no effect

on firm performance, or whether the negative correlation between ROA and covenant strictness (simultaneity) creates a downward bias for OLS estimates.

The results change considerably once we focus on variation in covenant strictness that comes from the number of defaults in the lender's loan portfolio, and is therefore plausibly exogenous to firm characteristics. Column 4 of Table 1.3 presents the results of the full sample IV/2SLS regressions. The effect of covenant strictness on profitability is positive and economically large. To evaluate the relative magnitude, an increase in covenant strictness by 10%, which is equivalent to the difference in strictness between A- and BBB-rated firms, leads to a 1.3% increase in profitability.

I also report the p-value from the Anderson-Rubin (AR) (1949) Wald test, which is robust to weak instruments. The null hypothesis in this test is that the IV is not relevant, and therefore, rejecting the null becomes harder as the instrument becomes weaker. In this sense, the AR p-value in column 4 of Table IV implies that we can reject the null hypothesis that the instrument is not relevant and weak.[11]

Covenant Strictness and Borrower-Lender Relationship

When a lender makes covenants on a new loan more restrictive after experiencing a larger number of defaults in its loan portfolio, borrowers could choose another lender

[11] Stock and Yogo (2005) simulate critical values based on which we can reject the null that an instrument is weak if the minimum eigenvalue of the first-stage F-statistic matrix, known as the Cragg-Donald F-Statistic, is above these critical values. The Cragg-Donald F-statistic in column 4 is 9.2, which lies between the 10%-15% thresholds reported in Stock and Yogo (2005) (see Table 5.2) and implies the instrument would be considered weak if we limited the size of an IV Wald test to at most 0.1 above its nominal value. The evidence in this section suggest that the instrument is not obviously weak.

offering slightly looser covenants. Why do borrowers, whose performance has not changed, accept stricter covenants instead of seeking a different lender?

A potential reason for borrowers to accept stricter covenants from their lender is that firms want to maintain their existing relationship with their lender. Banks that have a past credit relationship with a firm have private information about theses firms. As a result, due to the adverse-selection problem, switching to another lender is costly for firms (see Greenbaum et al. (1989), Sharpe (1990), and Rajan (1992)). For instance, Petersen and Rajan (1994, 1995) show that borrowing from many different lenders increases the cost of debt and reduces the amount of credit available to firms, whereas Bharath et al. (2011) find that firms obtain better loan terms by borrowing from a lender with an existing relationship. The key result in Boot (2000) and Boot and Thakor (2000) is that relationship lending is immune to banking competition and mostly affects transaction lending.

If borrowers with good credit quality pick lenders that offer looser covenants, the estimates from the full-sample regressions are likely to be biased downward. I hypothesize that stricter covenants should have a larger effect on the operating efficiency of firms that have an existing relationship with their creditor, because these firms are less likely to switch to a different lender after banks start making covenants more restrictive. To proxy for relationship lending between firms and the lead arranger, I restrict my attention to the group of firms that have borrowed at least twice from the same lead arranger in the past 5 years.

In Table 1.3, I show the regression results for the group of firms that have an existing relationship with a bank. Columns (2) and (3) present the estimation results

of OLS regressions in the subsample of firms that have an existing relationship with their creditor, using industry and firm fixed effects, respectively. The OLS regression results within this subsample are similar to the full-sample OLS estimates in equation (1); debt covenant strictness does not appear to affect firm performance.

In columns (5)-(6), the results of the IV/2SLS regressions are considerably different. Focusing only on the group of firms that maintain their lending relationships, I find the economic effect of strictness on profitability increases considerably. Specifically, a 10% increase in strictness leads to a 2.5% increase in profitability over the next four quarters. The AR p-value in column 4 is such that we reject the null that the instrument is not relevant and weak at the 1% level, whereas the Cragg-Donald statistic is 11.37 and the Kleibergen-Paap Lagrange multiplier F-statistic is 38.233.[12] Overall, these statistics suggest the regressions do not suffer from the weak instrument problem. Using firm fixed effects in column (6), the results remain robust. Specifically, an increase in debt covenant strictness above the firm's average has a statistically and economically large positive effect on firm performance. Taken together, the results in Table 1.3, suggest that stricter covenants have a positive effect on firm performance.

The Effects of Covenant Strictness on Long-Run Profitability

The empirical evidence suggests stricter debt covenants lead to an increase in firm profitability one year after loan origination. However, strict debt covenants may benefit the firm in the short-term, but at the expense of long-run profitability. I

[12]According to Stock and Yogo (2005), the critical values for the Cragg-Donald statistic at the 5% significance level is 8.96 when the worst bias relative to OLS estimates is 15%. Therefore, I can only reject the null that the bias relative to OLS estimates is 15% or more.

investigate this possibility in Table 1.4, by testing the effects of debt covenant strictness on firm performance two and three years after the loan origination date. Columns 1-3 in Table 1.4 present the respective results of OLS regressions of firm profitability after one year, two years, and three years on debt covenant strictness. Columns 4-6 present the respective estimates from IV/2SLS regressions.

The regression results in Table 1.4 suggest stricter covenants have a positive effect on firm profitability one year after the origination of the loan, but have no significant long-run effects on firm performance. Because firms renegotiate their loan terms frequently, covenant thresholds could change after renegotiation, which is consistent with a lack of long-run effects of covenants on firm performance. For instance, Roberts and Sufi (2009b) argue that almost 90% of loans are renegotiated prior to their stated maturity leading to considerable amendments to their initial terms. Denis and Wang (2014) present similar evidence, and find renegotiations can lead to either more restrictive or looser contract terms, depending on the post-renegotiation environment and the performance of the firm. The ability to renegotiate loan contract terms highlights the special role of private debt agreements in mitigating information asymmetry between lenders and borrowers, by setting tight loan covenants ex ante, and relaxing them ex post through loan renegotiations (see Garleanu and Zwiebel (2009)).

1.3.3 The Effects of Covenant Strictness on Cost Efficiency and R&D

The evidence so far suggests an increase in debt covenant strictness has a positive effect on profitability. In this section, I investigate in more detail how stricter covenants

affect profitability. The DuPont identity is a helpful starting point in this analysis. Specifically, I break down profitability (EBITDA/Assets) into two components, profit margins (EBITDA/Sales) and asset turnover (Sales/Assets), and then test how exogenous variation in covenant strictness affects these efficiency measures.

The regression results in the first two columns of Table 1.5 suggest the increase in profitability is driven by an increase in profit margins. The coefficient of covenant strictness in the first column is 0.0015 and indicates a 10% increase in strictness leads to a 1.5% increase in profit margins. However, covenant strictness does not affect asset turnover. One explanation is that changes in asset turnover require time and long term-adjustments for the firm. Therefore, it seems intuitive that changes in profit margins drive the observed effect on profitability, at least in the short run, because firms can more easily improve their performance either by increasing prices or by reducing cost.

To verify whether firms indeed reduce cost, I also study how covenants affect two of the most common measures of cost efficiency: cost of goods sold over sales, and administrative expenses over sales. I find that stricter contracts do not have a significant effect on administrative expenses, but they reduce the cost of goods sold. In particular, a 10% increase in debt covenant strictness leads to a 1.3% decrease in operating cost. The empirical evidence suggests therefore that debt covenants increase profitability mostly by reducing cost. However, although statistically weak, the regression in column (5) suggest that stricter covenants also have a negative effect on R&D expenses. Specifically, one within-firm standard deviation increase in

covenant strictness ($\approx 10\%$) leads to a 0.58% reduction in R&D, or a reduction of one third of its standard-deviation.

I do not find evidence that stricter loan covenants affect wages, advertising, and employment.[13] Falato and Liang (2016) find that covenants violations lead to a reduction in employment. The results in this study suggest, unlike technical violations of financial covenants that provide the first signal of financial distress, the tightening of covenants at origination may not yet necessitate significant changes in more persistent variables such as wages or employment.

1.3.4 The Effects of Covenant Strictness on Earnings Manipulation and Risk Shifting

To ensure compliance with stricter covenants and avoid a costly violation, managers may try to manipulate earnings to show temporary earnings or sale increases (see DeFond and Jiambalvo (1994) Burgstahler and Dichev (1997)). In this case, tighter covenants may not necessarily cause an increase in operating efficiency, but, instead, managers may manipulate earnings to loosen credit constraints. However, it is difficult to test this hypothesis directly because banks could impose more restrictive covenants on firms that are more likely to manipulate earnings. To address this simultaneity, I use exogenous variation in covenant strictness to test directly whether stricter covenants lead to an increase in earnings management.

To test whether firms engage in earnings management, I estimate discretionary current accruals using Jones (1991), and its modified version from Dechow et al.

[13]Results are not tabulated in the main text, but are available in the appendix Table **??**.

(1995), and regress discretionary accruals on the IV for covenant strictness. The results in Table 1.6 suggest that an exogenous increase in covenant strictness does not have a significant impact on earnings management.

I also investigate whether stricter covenants induce managers to increase the riskiness of the projects they undertake. To measure operational risk, I calculate the trailing standard deviation of four quarterly changes in ROA. Although relatively noisy, this measure derives risk based on profitability instead of financial prices. The evidence in Table 1.7 suggest that strictness covenants also lead to a reduction in operational risk, although the effect becomes statistically significant only after six quarters.

Taken together, the results in this section are not consistent with the managerial myopia hypothesis. First, the positive effects of covenants on performance do not reverse after two or three years (see Table 1.4), and there is no evidence suggesting that managers manipulate earnings, or take on more risk.

1.4 The Governance Role of Debt

1.4.1 The Interplay of Debt Governance with Corporate Governance Mechanisms

The positive effects of loan covenants on operating performance raise the following question: if stricter loan covenants improve profitability, why firms do not demand stricter debt covenants in the first place? The optimal set of financial covenants

should balance agency conflicts between creditors and shareholders in a way that maximizes the value of the firm (Smith and Warner (1979)).

Managers, however, who negotiate the strictness of loan covenants with creditors, have an incentive to set covenants looser than what would be optimal for the firm to protect their job, maintain their flexibility, and maximize their personal utility (Donaldson (1963) and Myers (1977)). Due to the separation of ownership and control, therefore, it is possible that managers might select debt contract terms that may not be optimal for the firm. Consequently, changes in covenant strictness could lead to significant efficiency gains for the firm, especially when firms lack alternative governance control mechanisms to protect shareholders' interests.

The corporate governance literature has numerous examples that highlight how agency conflicts between managers and shareholders lead to considerable inefficiencies and reduce firm value (Bertrand and Mullainathan (2003), Bebchuk and Cohen (2005), Chhaochharia and Grinstein (2007), Cohen and Wang (2013)). I hypothesize that if creditors promote corporate efficiency with stricter covenants by disciplining managers, covenants should mostly affect firms that lack alternative governance mechanisms to protect shareholders' interests (poorly governed firms). This hypothesis is based on theoretical and empirical evidence that highlights the monitoring and disciplining role of debt.[14]

[14]Without being exhaustive, the following papers discuss the role of covenants in monitoring and aligning the incentives of the manager with shareholders: Holmstrom and Tirole (1997), Park (2000), Dichev and Skinner (2002), Gorton and Winton (2003), Ryan and Wiggins (2004), Roberts and Sufi (2009b), and Rauh and Sufi (2010)).

To test this hypothesis, I split the sample into two groups: firms with high and firms with low agency conflicts between managers and shareholders. Specifically, a firm is poorly governed (i.e., has high agency conflicts between shareholders) in the following cases: the fraction of its shares owned by blockholders is below the sample median; the number of anti-takeover provisions is above the sample median; the firm's concentration in its product market is above the median; and the share of independent directors is below 50%. I examine the effect of debt covenant strictness on profitability separately for well-governed and poorly governed firms, and present the results of all regressions in Table 1.8.

Governance Mechanism: Blockholder ownership

I first investigate the effect of covenant strictness on profitability of firms with high and firms with low institutional blockholder ownership. Blockholders are large shareholders that own more than 5% of the firm's outstanding shares. Institutional and large investors are usually active shareholders that have a greater incentive to monitor managers and are more likely to intervene when managers do not run the firm efficiently (see Shleifer and Vishny (1986), La Porta et al. (2002), and Gillan and Starks (2007)). Therefore, high institutional blockholder ownership is a common proxy in the corporate governance literature for institutional monitoring, or high internal governance (see Cremers and Nair (2005), Cremers et al. (2007), Chen et al. (2007), Edmans (2009), and Fich et al. (2015)). If stricter debt covenants have a positive effect on firm profitability due to increased monitoring of the manager,

increasing covenant strictness should have a larger effect on profitability of firms without large shareholders.

The results of Table 1.8 suggest an increase in covenant strictness has a large positive effect on profitability when large shareholders are not present. In the first column of Panel A in Table 1.8, the effect of debt covenant strictness on firm profitability is almost twice as large if we focus only within firms with low blockholder ownership. Specifically, I find that in firms whose institutional blockholder ownership is below the sample median, a 10% increase in covenant strictness leads to a 2.1% increase in profitability one year after the loan. Moreover, if stricter debt covenants increase firm efficiency and profitability because of the increased monitoring of management, stricter debt covenants should not affect firms that already have a strong form of internal governance. The results in Panel B, Table 1.8, are consistent with this hypothesis, suggesting an increase in covenant strictness does not have a statistically significant effect on profitability of firms with high blockholder ownership.

Governance Mechanism: Product Market Competition

I also hypothesize that managers are less likely to improve efficiency when they are insulated from competitive threats (see Giroud and Mueller (2011)), and therefore an exogenous increase in covenant strictness should have a larger effect on these firms. According to Shleifer and Vishny (1997), competition is one of the strongest forms of corporate governance mechanisms, forcing firms to minimize costs and improve efficiency. To this end, I split firms into two groups based on the level of competition in their product market. To proxy for competition, I use a text-based measure of

competition from Hoberg and Phillips, *TNIC HHI* (see Hoberg and Phillips (2010a), Hoberg and Phillips (2010b)).[15]

Table 1.8 (third column) presents the results of IV/2SLS regressions of firm profitability on debt covenant strictness for the subsample of firms that operate in product markets with low competition (Panel A) and high competition (Panel B). The results for the low-governance subsample in Panel A suggest a 10% increase in strictness leads to a 2.8% increase in firm profitability. On the contrary, in Panel B, an increase in debt covenant strictness for firms that operate in highly competitive product markets does not yield an economically large or statistically significant effect on firm profitability. In this sense, the results in the third column of Table 1.8 suggest debt covenant strictness acts as a corporate governance substitute, and are similar to Giroud and Mueller (2010, 2011), who show corporate governance mechanisms matter more when competition in firms' product market is lacking.

Governance Mechanism: Shareholder Rights

Gompers et al. (2003) provide evidence that firms with more shareholder rights perform better, and Bertrand and Mullainathan (2003) find that when managers are protected from takeovers, they tend to run firms less profitably and less efficiently. Therefore, if strict debt covenants act as a disciplining device for managers, stricter debt covenants should have a larger effect on firms with more anti-takeover provisions

[15]Unlike traditional industry classifications, Hoberg and Phillips use textual analysis based on 10K reports to define industries based on the product description of each firm. Using these more refined definitions of industries, which are updated yearly and have the same coarseness as SIC-3 industry classification, the authors calculate the Herfindahl Hirchman index (HHI).

and fewer shareholder rights. To test this hypothesis, I collect data on the number of governance provisions for each firm (G-index). The governance index, first constructed by Gompers et al. (2003), is a count measure from 1-24 based on the number of anti-takeovers provisions and the (lack of) shareholder rights.[16] Matching data on shareholder rights significantly reduces my sample, because a large portion of firms in Dealscan are private.

In the second column of panels A and B of Table 1.8, I test whether strict debt covenants have a larger effect on performance if managers are entrenched from takeovers. I group firms in high- and low-governance groups based on whether the governance index is above or below the sample median. The IV/2SLS regression in panel A suggests an increase in debt covenant strictness has a positive effect on profitability, but the effect is statistically significant only for firms whose G-index is above the median.

Governance Mechanism: Board Independence

I next investigate whether stricter debt covenants have a larger effect on performance of firms whose board is dominated by inside directors. Monitoring the manager is the most important task for outside directors (Byrd and Hickman (1992)). I hypothesize that if the majority of directors are not independent (outsiders), managers are less likely to optimize firm performance, and therefore an increase in debt covenants strictness should matter more for this type of firms. Specifically, in the last column of Table 1.8, I test how variation in debt covenant strictness affects firms with a majority

[16]Andrew Metrick has generously made the data available on his website.

of inside directors (panel A) and firms with a majority of independent directors (panel B). The results suggest an increase in debt covenant strictness does not have a significant effect on the profitability of firms with a majority of independent directors (>50%). Focusing on the subsample of firms with a majority of inside directors changes the results considerably. Specifically, I find a 10% increase in debt covenant strictness leads to a 1.9% increase in profitability, although the effect is statistically significant only at the 10% level. These results complement the previous findings and are consistent with the idea that debt covenants act as a governance substitute, leading to a positive and economically large increase in firm performance.

1.4.2 The Effects of Covenant Strictness on Performance Through Incentives

Previous studies examine the association between covenant violations and firm outcomes. For instance, Chava and Roberts (2008), Roberts et al. (2009), and Nini et al. (2012) find that covenant violations allow creditors to intervene with firms' financial policy and affect their performance. Covenant violations are also associated with a reduction in employment and establishment closures (see Falato and Liang (2016) and Ersahin et al. (2016)).

This paper is different from previous empirical studies focusing on the effects of covenant violations. By excluding covenant violations and the effects of creditor intervention on firm policies, I use exogenous variation in covenant strictness at loan origination to investigate the treatment effect of covenants through incentives. Although the evidence in the previous section are consistent with this interpretation,

I also use a more direct method to test whether covenants affect firm performance through incentives.

To test this hypothesis I identify whether a loan package contains performance pricing terms. Performance pricing terms link the cost of debt with firm performance; when firms perform poorly interest rates increase, and when firm performance improves interest rates decline. Creditors use performance pricing as a "carrot and stick" incentive mechanism to motivate managers to exert higher effort (see Manso et al. (2010)). Therefore, stricter covenants should have a larger effect on firms whose loans do not have performance-linked spreads.

In Table 1.9 I split firms into two groups, based on whether their cost of debt is linked with the debt to cash flow ratio, or interest coverage ratio. The regression results support the hypothesis that stricter covenants affect performance through incentives. Specifically, an increase in covenant strictness leads to a statistically significant effect on profitability only for firms whose contracts do not contain cash flow based pricing grids (column 2). This result, therefore, is consistent with the hypothesis that stricter covenants affect firm efficiency through incentives.

1.5 Robustness

1.5.1 Matching

As a source of exogenous variation in covenant strictness, I use the number of defaults on the portfolio of the lead arranger recently prior to making a new loan. To construct the IV, I calculate for each bank in the sample the number of firms that defaulted on

a bond payment. Unlike violations on private agreements, large defaults on public debt are important events for a lender that significantly affect its assessment of its screening and monitoring process. However, this methodology creates an imbalance between firms that borrow from a lead arranger that has suffered at least one default (treated group) and firms that borrow from a lead arranger that is less likely to have made loans to firms that issue bonds (control group). Smaller banks make loans to smaller firms, which are less likely to issue bonds. As a result, firms in the treated group, or firms whose lenders tend to experience defaults in their portfolio, are more likely to be larger and have different characteristics than firms in the control group (firms whose lenders suffer no defaults).

Does the firm-bank match possibly generate a selection bias that drives the results in this paper? Although I explicitly control for firm and bank characteristics in the regressions, to further address this concern, I combine IV regressions with fixed effects on a matched sample. Specifically, I match firms whose lead arranger in the six-month period leading to the new loan had experienced at least one default (treated group) with firms whose lead arranger had not suffered from any defaults (control group), on the basis of the following characteristics: firm size, book leverage, market-to-book value, past profitability, lead-lender capitalization, and assets. Barber and Lyon (1996) argue that test statistics of empirical methods based on measures of operating performance are well specified when treated firms are matched to a control group of similar pre-treatment performance. Matching firms on these characteristics following the methodology of Abadie and Imbens (2006), I limit my sample in the common support of the treated and control group, and use IV/2SLS regressions to estimate

the effect of covenant strictness on firm performance. The second panel of Table 2.9 verifies that firms in the treated and control group have similar characteristics, including past performance, and therefore these basic covariates are well balanced.

Focusing only on the matched sample, the top panel of Table 2.9 presents the results from the OLS regression of profitability on covenant strictness (first column) and instrumented covenant strictness (right column). Consistent with the evidence in Table 1.3, an increase in covenant strictness has a positive effect on profitability only once I address the endogeneity between covenant strictness and firm efficiency using an IV. Overall, the results in the matched sample confirm the empirical findings in the previous sections, and suggest an increase in covenant strictness has an economically and statistically significant positive effect on profitability, even after addressing potential confounding factors attributed to significant differences between firms in the treated and control groups.

1.5.2 Robustness Tests

I test whether the IV/2SLS methodology spuriously generates a positive effect of covenant strictness on operating performance. I randomly assign defaults in the portfolio of the lead arranger, and simulate 1,000 regressions of profitability on covenant strictness (instrumented) as in column 4 of Table 1.3. Because the IV is random, we should see a statistically significant change in profitability in fewer than five out of 100 simulations (to satisfy the 5% confidence interval). Out of 1,000 simulations, the statistic is greater than 1.96 in only seven instances (Figure 1 in the online Appendix shows the distribution of t-statistics). The simulation results suggest

the statistical significance of the IV/2SLS results fall within the desired confidence interval, and the observed results in the previous sections are not the outcome of chance.

I also perform a Durbin-Wu-Hausman test (as seen in Hausman (1978)) to examine if estimates from OLS regressions are biased. The results from this test determine whether we need an instrument to identify the relationship between covenant strictness and profitability. To perform this test, I take the residuals from the first-stage regression of covenant strictness on the IV (the defaults count measure), and then include the residuals as a control variable in a regression of profitability on debt covenant strictness. If a significant correlation exists between the unexplained portion of covenant strictness and profitability in the second stage, it would strongly suggest the presence of endogeneity. In Table 1.11, the residuals from the first-stage regression have a significantly negative correlation with profitability. This result implies the unexplained portion in covenant strictness in a normal OLS framework will cause a downward bias in our estimates.

1.6 Conclusion

In this paper, I estimate the causal effects of restrictive loan covenants on firm profitability. As an instrument for covenant strictness, I use the number of defaults in the portfolio of the lead arranger leading up to the origination of a new loan. Focusing on syndicated loan contracts, a major source of external finance for firms, I find stricter debt covenants cause an economically and statistically large increase in

profitability, and a reduction in operating cost. However, stricter covenants improve firm performance only when large shareholders have no presence on the firm's board, when managers are entrenched, when inside directors dominate the board, or when firms face softer competition in their product market. The results support the view that stricter debt covenants act as a governance substitute and mitigate agency costs generated by the separation of ownership and control.

To my knowledge, this attempt is the first to estimate the causal effect of loan covenants on firms' operating efficiency. The main advantage of the IV approach is that it allows us to generalize the results to a broader spectrum of firms and does not limit the external validity of the results to only a specific type or group of firms. For instance, previous studies exploit threshold discontinuities—such as covenant violations—and estimate the effects of creditor intervention on firm policies on the subsample of poorly performing firms that violate their covenants. Creditors could affect firm performance not only through direct intervention, but also by putting the business under a shorter a leash and increasing the threat of technical violation with more restrictive debt covenants. The IV in this paper provides exogenous variation in the design of covenants at origination across a broad spectrum of firms, and allows us to identify hidden and potentially important economic relationships between the design of incentive contracts and firm performance.

1.7 Tables Appendix

APPENDIX: Definition of Variables
This table provides details for the variables used throughout the paper. Accounting data are from Compustat's quarterly file, and loan data are from LPC Dealscan. I winsorize all variables at the 1st and 99th percentile.

Variable Names	Description
Defaults (180 days)	The number of firms whose S&P credit rating changed to default or selective default while they still had an active loan 180 days prior to a new loan made by their lead arranger.
	Firm Characteristics
Book Leverage	Total Debt / Book Assets
Current Ratio	Current Assets / Current Liabilities
Debt/Tangible Net Worth	Total Debt / Tangible Net Worth
EBITDA	Sum of rolling four-quarter operating income before depreciation
EBITDA / Assets	EBITDA / Book Assets (ROA)
Fixed Charge Coverage	EBITDA / (Rolling 4-quarter Interest and Related Expenses + Debt in current liabilities one year prior)
Market Equity	Stock Price × Shares Outstanding
Market-to-Book	(Market Equity + Total Debt + Preferred Stock liquidating value - Deferred Taxes and Investment Tax Credits) / Book Assets
EBITDA / Sales	EBITDA / Sum or rolling four-quarter sales
Sales/Assets	Sum of rolling four-quarter sales / Book Assets
Tangible Net Worth	Net PPE / Book Assets
Total Debt	Short-term debt + Long-term debt

Z-Score	3.3×Pre-tax Income/Assets + 0.999×Sales/Assets + 1.4 ×Retained Earnings/Assets + 1.2 × (Current Assets - Current Liabilities)/Assets +0.6×Mkt Equity/Total Liabilities
	Loan Characteristics
Loan Spread	The All-in-Drawn Spread from Dealscan, for each dollar borrower draw, excluding fees
Covenant Number	Total number of financial covenants in the loan contract
Loan Amount	The sum of all amounts in a certain loan package
Loan Maturity	The number of months between the earliest loan origination date and the latest maturity date in a certain loan package
Loan Purpose	Indicator variables for the following categories reported in DealScan: corporate purposes, debt repayment, working capital, takeover, CP backup, or other
Loan Participants	The total number of participating banks (non lead-arrangers) in a certain loan package
Covenant Strictness	Indicates the probability that the firm will violate at least one of its covenants in the next quarter. To construct the measure, I follow the methodology of Murfin (2012). To construct the measure I use covenant definitions from Demerjian and Owens (2014) and also include additional ratios often used in debt contracts
	Governance Characteristics
Blockholder Ownership	The share of a firm's shares owned by shareholders with at least 5% of the firm's outstanding shares (Source: Thomson Reuters 13F)
G-Index	Governance index from Gompers et al. (2003). The measure ranges from between 1-24, with higher values representing lower shareholder rights

TNIC-3-HHI	Measure of product market competition constructed by Hoberg and Phillips (2010a) based on firm pairwise similarity scores from text analysis of firm 10K product descriptions. Data are available at `http://cwis.usc.edu/projects/industrydata/`.
Board Independence	The share of independent directors in the board of a company (Source: Boardex)
	Bank Characteristics
Bank Assets	Bank Assets (Sources: Compustat NA Bank, Compustat Global, Bankscope)
Bank Capitalization	Shareholder equity / Bank assets (Sources: Compustat NA Bank, Compustat Global, Bankscope)

Table 1.1: **Summary Statistics**
This table presents summary statistics of firm, loan, governance, and bank characteristics. Accounting information is from Compustat. Loan information is from LPC Dealscan. Bank information is from Compustat NA Bank, Compustat Global, and Bankscope. Governance variable are from various sources cited in the Appendix. All variables are described in detail in the Appendix.

Variable	**N**	**Mean**	**SD**	**10%**	**50%**	**90%**
			Firm Characteristics			
Total Assets(m.$)	3701	3381.8	6482.1	157.0	1184.8	8418.1
EBITDA/Assets	3701	0.15	0.07	0.07	0.14	0.23
Market Value/Book Value	3701	1.42	0.93	0.64	1.16	2.44
Has SP rating	3701	0.50	0.50	0.00	1.00	1.00
Tangibility	3700	0.35	0.24	0.08	0.29	0.74
Book Leverage	3701	0.27	0.15	0.07	0.26	0.47
Altman-Z	3701	4.75	3.00	1.40	4.31	8.86
			Loan Variables			
Cov.Strictness	3701	22.79	20.79	0.04	18.31	52.91
Maturity (months)	3701	49.92	18.75	21.00	60.00	61.00
Amount(m.$)	3701	475.20	679.14	40.00	250.00	1100.00
Secured	3701	0.47	0.50	0.00	0.00	1.00
Number of Participants	3701	9.76	8.14	1.00	8.00	21.00
Number of Lead Arrangers	3701	1.57	1.06	1.00	1.00	3.00
			Governance Variables			
Blockholder Ownership	2720	0.18	0.14	0.00	0.17	0.37
G-index	1643	9.16	2.57	6.00	9.00	13.00
TNIC-3-HHI	3135	0.20	0.20	0.04	0.13	0.47
Independent Directors(%)	1423	0.56	0.26	0.11	0.64	0.83
			Bank Characteristics			
Bank Assets(b.$)	3409	843.6	754.9	111.0	619.9	2210.9
Bank Capitalization	3409	0.078	0.022	0.05	0.080	0.107

Table 1.2: **First-Stage - OLS regressions of covenant strictness on lender defaults**

This table presents the estimation results from OLS regressions of covenant strictness. In all regressions the dependent variable is covenant strictness of the loan contract; it ranges from 0-100 and represents the probability that the firm will violate at least one covenant over the next quarter. To construct the loan covenant strictness measure I follow Murfin (2012) (see the Appendix Table for detailed explanation of its construction). The independent variable of interest is the number of outstanding loans in the portfolio of the lead arranger that defaulted 180 days prior to the origination of the new loan. In all regressions, I include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	Cov Strictness	Cov Strictness	Cov Strictness
Lender Defaults (past 180 days)	0.893***	0.825***	0.908***
	(0.207)	(0.217)	(0.302)
$(\text{EBITDA/Assets})_{t-1}$	-54.093***	-55.214***	-58.918***
	(4.787)	(6.640)	(8.800)
Ln(Assets)	-3.088***	-0.679	1.630
	(0.571)	(0.667)	(1.983)
Book Leverage	45.807***	48.504***	50.999***
	(2.741)	(4.123)	(5.938)
Altman Z-score	-0.557***	-0.431**	-0.223
	(0.211)	(0.218)	(0.410)
Observations	3379	3379	3379
Adjusted R^2	0.299	0.390	0.560
Firm-Bank Controls	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	No	Yes	Yes
Industry FEs	No	Yes	No
Firm FEs	No	No	Yes

Table 1.3: **OLS vs IV/2SLS regressions of firm profitability on covenant strictness**
This table presents the estimation results from OLS (1-3) and IV/2SLS (4-6) regressions of changes in profitability (ROA). The dependent variable is the change in EBITDA/Assets one year after the loan origination date. The independent variable of interest in OLS regressions is the strictness of debt covenants (see the Appendix for measure details). In IV/2SLS regressions, the instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. Columns (1) and (4) use the full sample of loans, and columns (2)-(3) and (5)-(6) use the sample of firms that have an existing relationship with the lead lender (at least two loans). In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). All regressions include year, lender, purpose, and rating fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	OLS			**IV/2SLS**		
	(1) ΔROA_{t+1}	(2) ΔROA_{t+1}	(3) ΔROA_{t+1}	(4) ΔROA_{t+1}	(5) ΔROA_{t+1}	(6) ΔROA_{t+1}
Cov.Strictness	-0.0000	-0.0000	-0.0002			
	(0.0001)	(0.0001)	(0.0001)			
Cov.Strictness (Instr)				0.0013**	0.0025**	0.0027**
				(0.0007)	(0.0010)	(0.0013)
Observations	2969	1466	1466	2969	1466	1466
Adjusted R^2	0.182	0.255	0.464	-	-	-
AR p-value	-	-	-	0.001	0.00	0.001
Firm-Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	No	Yes	Yes	No
Firm FEs	No	No	Yes	No	No	Yes

Table 1.4: **IV/2SLS regressions of firm profitability (year +1,+2,+3) on covenant strictness**
This table presents the estimation results from OLS and IV/2SLS regressions of changes in profitability (ROA) one, two, and three years after loan origination, respectively. The independent variable of interest is an instrument for the strictness of debt covenants (for details, see the Appendix). The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the loan origination date. In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). All regressions include year, lender, purpose, and rating fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	OLS			**IV/2SLS**		
	(1)	(2)	(3)	(4)	(5)	(6)
	$\Delta(\text{ROA})_{t,t+1}$	$\Delta(\text{ROA})_{t,t+2}$	$\Delta(\text{ROA})_{t,t+3}$	$\Delta(\text{ROA})_{t,t+1}$	$\Delta(\text{ROA})_{t,t+2}$	$\Delta(\text{ROA})_{t,t+3}$
Cov.Strictness	-0.0000	-0.0000	-0.0005**			
	(0.0001)	(0.0001)	(0.0002)			
Cov.Strictness (Instr)				0.0013**	0.0014	0.0023
				(0.0007)	(0.0013)	(0.0019)
Observations	2969	2746	2080	2969	2746	2080
Firm-Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.5: **The effect of covenant strictness on cost efficiency and R&D expenses**
Starting from the regression on the left to the right, the dependent is: change in EBITDA over sales (profit margins), change in sales over assets (asset utilization), change in cost of goods sold over sales, change in administrative expenses over sales, and change in R&D expenses over assets. In column (5), I multiply the change in R&D expenses by 100, so the estimated coefficients represent the percentage change in R&D over assets. In all columns, the change in the dependent variable is based on the date of loan origination and the year after. The instrumental variable is the number of defaults the lender has suffered in its portfolio 180 days prior to the loan origination date. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). All regressions include year, lender, purpose, and rating fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)	(4)	(5)
	(Ebitda/Sales)	(Sales/Assets)	(Cost of goods/Sales)	(Adm.Exp./Sales)	(R&D/Assets)
Cov.Strictness (Instr)	0.0015**	-0.0016	-0.0013***	0.0010	-0.0577*
	(0.0007)	(0.0054)	(0.0004)	(0.0014)	(0.0329)
Ln(Assets)	-0.0017	0.0334***	-0.0031*	-0.0040*	0.0329
	(0.0021)	(0.0109)	(0.0017)	(0.0022)	(0.1623)
Book Leverage	-0.0196	0.1728	0.0541***	-0.0512	2.1905
	(0.0373)	(0.2373)	(0.0115)	(0.0623)	(1.6307)
Altman Z-score	0.0064***	-0.0152**	-0.0037***	-0.0005	0.0352
	(0.0010)	(0.0077)	(0.0011)	(0.0016)	(0.0409)
Observations	2981	3052	3027	2696	1127
Firm-Bank Controls	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes

Table 1.6: **Do stricter debt covenants affect earnings manipulation?**
The dependent variable in column (1) is discretionary current accruals as in Jones (1991) method. In column (2), the dependent variable is discretionary current accruals using the modified Jones (1991) model as in Dechow et al. (1995). The independent variable is an instrument for the strictness of debt covenants. The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Discretionary Cur. Accruals (Jones method)	(2) Discretionary Cur. Accruals (Modified Jones)
Cov.Strictness (Instr)	0.005	0.006
	(0.006)	(0.007)
Observations	2871	2866
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Table 1.7: **Do stricter debt covenants affect risk shifting?**
The dependent variable in all regressions is the trailing standard deviation of four quarter changes in Ebitda/Assets (ROA). In columns (1)-(3), the dependent variable is the trailing standard deviation of quarterly changes in ROA four quarters, six quarters, and eight quarters after the origination of the loan, respectively. The independent variable is an instrument for the strictness of debt covenants. The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	ROA Volatility(4qtr)	ROA Volatility(6qtr)	ROA Volatility(8qtr)
Cov.Strictness (Instr)	-0.0015	-0.0041**	-0.0052**
	(0.0016)	(0.0021)	(0.0022)
Observations	3001	2856	2752
Firm-Bank Controls	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes

Table 1.8: **OLS vs. IV/2SLS regressions of firm efficiency on covenant strictness. Low Governance Firms (Panel A) vs High Governance Firms (Panel B)**

This table presents the estimation results from IV/2SLS regressions of changes in profitability (ROA). The dependent variable in all regressions across both panels is the change in EBITDA/Assets one year after the loan origination date. The independent variable of interest is an instrument for the strictness of debt covenants. The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. Regressions in Panel A contain only firms that at the date of loan origination have low governance, and in Panel B firms with high governance. A firm is poorly governed if its ownership from large shareholders is below the median (*Blockholders Low*), shareholder rights are low (*G-index high* below median), competition in its product market is below the median (*Competition Low*), and the share of independent directors on the board is less than 50% (*Board independence high*). In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Panel A: Low Governance Firms			
	(1)	(2)	(3)	(4)
	(Blockholders Low)	*(G-index High)*	*(Competition Low)*	*(Board Indep. Low)*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness (Instr)	0.0021**	0.0024**	0.0028**	0.0019*
	(0.0011)	(0.0010)	(0.0011)	(0.0011)
Observations	981	828	1397	348
Firm-Bank Controls	Yes	Yes	Yes	Yes
Year,Purpose,Rating FEs	Yes	Yes	Yes	Yes
Lender FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes

(Table 1.8—*Continued*)

	Panel B: High Governance Firms			
	(1)	(2)	(3)	(4)
	(Blockholders High)	*(G-index Low)*	*(Competition High)*	*(Board Indep. High)*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness (Instr)	-0.0025	-0.0002	-0.0010	-0.0009
	(0.0018)	(0.0015)	(0.0020)	(0.0009)
Observations	1249	590	1265	926
Firm-Bank Controls	Yes	Yes	Yes	Yes
Year,Purpose,Rating FEs	Yes	Yes	Yes	Yes
Lender FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes

Table 1.9: **OLS vs IV/2SLS regressions of firm profitability on covenant strictness - Performance Pricing**

This table presents the estimation results from IV/2SLS regressions of changes in profitability (ROA). The dependent variable is the change in EBITDA/Assets one year after the loan origination date. The independent variable of interest in is strictness of debt covenants (see the Appendix for measure details). The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. Column (1) contains only firms whose loan spread is linked to their debt to cash flow ratio, or interest coverage ratio. Column (2) contains only firms whose loan does not contain performance pricing terms based on debt to cash flow or interest coverage. In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)
	(With Performance Pricing)	*(Without Performance Pricing)*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness (Instr)	0.0012	0.0021**
	(0.0015)	(0.0009)
Observations	1293	1675
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Table 1.10: **OLS and IV/2SLS regressions of profitability on covenant strictness in a matched sample**
The top panel of this table presents the estimation results from OLS (left column) and IV/2SLS (right column) regressions of changes in firms' profitability one year after the loan origination date on covenant strictness (left column) and its instrument (right column), respectively. I match treated firms (whose principal bank has suffered at least one default in the past 180 days prior to making a loan) with a group of control firms (principal bank has suffered no defaults in the past 180 days prior to the loan origination date) based on size, book leverage and market to book, lagged ROA, lead-lender capitalization, and assets. The bottom panel presents the standardized differences of these variables across treated and control units. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Matched Sample	
	OLS	*IV/2SLS*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov Strictness	0.0001	
	(0.0001)	
Cov Strictness (Instr)		0.0021**
		(0.0009)
Observations	1243	1243
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Covariate Balance	(Defaults=0)	(Defaults>0)	Standardized Diff.	p-value
Ln(Assets)	7.42	7.55	0.13	0.100
MB	1.39	1.45	0.06	0.294
BL	0.27	0.26	-0.01	0.112
ROA_{t-1}	0.15	0.15	0.00	0.895
Capitalization	0.07	0.07	0.00	0.236
Ln(Bank Assets)	13.55	13.54	-0.01	0.715

Table 1.11: **Hausman-Durbin-Wu test of endogeneity**
This table presents the estimation results from OLS regressions of changes in profitability, as in Table III (column 5). The dependent variable is the change in EBITDA/Assets a year after the start of the loan. The instrumental variable for contract strictness is the number of defaults in the lender's loan portfolio 180 days before the origination of the loan. The regression includes as an independent variable the residuals from the first-stage regression of contract strictness on the number of lenders' defaults. In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness	0.0016**
	(0.0008)
Strictness Residual (Hausman)	-0.0016**
	(0.0008)
Ln(Assets)	-0.0092**
	(0.0038)
Book Leverage	-0.0100
	(0.0385)
Altman Z-score	0.0110***
	(0.0009)
Observations	2976
Adjusted R^2	0.181
Firm-Bank Controls	Yes
Year,Lender,Purpose,Rating FEs	Yes
Industry FEs	Yes

— Chapter 2 —

Loan Syndication Networks and Loan Pricing - Evidence from Bank Mergers

2.1 Introduction

A small number of interconnected banks increasingly dominate the syndicated loan market. In 2014, the top 5 financial institutions had underwritten and jointly participated in more than 80% of all loans syndicated inside the United States. The syndicated loan market is a hybrid form of private and public financing, whereby lenders need to compete with each other to gain the coveted role of the lead arranger, but also need to collaborate to share the costs associated with the loan issue (Dennis and Mullineaux (2000)). Therefore, traditional measures of market concentration, such as the bank's market share, cannot capture the relationships, the connections, or the reciprocity that lenders develop with each other over years of collaborating in loan syndications. In this study, I use measures from network analysis to investigate whether lender interconnectedness helps explain the pricing and non-pricing terms of syndicated loans.

Theory suggests that lender interconnectedness may have either a positive or a negative effect on loan prices. On the one hand, well-connected underwriters can exploit their position in the banking industry to coordinate their activities and extract higher rents from borrowers via threat of adverse selection (see Sharpe (1990),

Rajan (1992), Hauswald and Marquez (2006)). On the other hand, well-connected underwriters can facilitate collaboration with other lenders and improve the efficiency of their screening and monitoring decisions, leading to a reduction in firms' cost of debt (Broecker (1990), Shaffer (1998), Marquez (2002)).

To measure the interconnectedness of a lender in the syndicated loan market, I use three measures of network centrality. These measures capture three different aspects of a bank's position in the network of lenders that originate and participate in syndicated loans. To calculate the first measure of centrality, degree centrality, I count the number of different investors that a lender has co-syndicated with over the past year to capture the breadth of its network of collaborators. To create the second measure of lender centrality, outdegree centrality, I count the number of different banks that participated in deals originated by that lender; this measure is different from degree centrality because it captures a lender's ability to find investors to participate in its own deals. Finally, for every banks I also calculate its indegree centrality, by counting the number of different lenders that invited this bank to participate in their deals; this measure captures a bank's ability to access information about borrowers they otherwise would not have access to. These centrality measures, unlike market share, are based on the propensity of lenders to create and participate in syndicated loans, and capture different dimensions of lenders' influence on the loan market.[1]

[1]Esty (2001) stresses the importance of maintaining close connections with other banks to compete for the role of the lead arranger by including a quote from loan office from Chase: *"The key to success in this business is being close to the market. This means being in touch with banks on a weekly, if not daily, basis. We started with a universe of 90 banks and created a target lender list*

I find evidence that, on average, highly interconnected lenders offer 10-15 basis points lower loan spreads to firms. Lenders with a large market share also offer lower interest rates, but the effect of market share on loan prices drops considerably when I include both interconnectedness and market share in the regressions. These findings suggest that measures of lender interconnectedness can better explain the variation in loan spreads.

The main challenge in testing the effects of lender interconnectedness on interest rates is that firms with lower credit risk may choose to borrow from lenders that are well-connected. To ensure that borrowers do not match endogenously with well-connected lenders, I focus on a sample of firms borrowing from a lender that was acquired by or merged with another bank. Focusing on this sample allows me to exploit variation in lender connectedness induced by bank mergers, and is plausibly exogenous to firm characteristics or bank-firm relationships.[2] For instance, Schlumberger, an oil gas services company, established a credit line with Chase in August 1999 and renewed its credit line with JP Morgan Chase after their merger in 2000. The identifying assumption is that the change in the connectedness of the lead arranger after the merger is exogenous to borrower characteristics because the firm had an existing relationship with one of the lenders.

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that might be interested in this deal. [..] Based on our analysis for the Disney deal, we expected it would be oversubscribed by 57%. This kind of analysis illustrates our closeness to the market and our confidence in the deal."

[2] Asker and Ljungqvist (2010) also use bank mergers as an exogenous shock to borrowers' information flows across banks. In Thakor and Boot (2008), Philip Strahan also argues that "The ideal empirical test would involve exogenous shocks to relationship duration, such as what might occur following a bank merger or failure".

Using variation in lead arranger centrality after a bank merger, I find that firms' cost of debt declines by 8 basis points on average, but only if the borrowers' lead arranger experiences a large increase in outdegree centrality. Interestingly, increases in market share, degree centrality, and indegree centrality do not appear to have a statistically significant effect on borrowers' cost of debt. These results corroborate the view that interconnectedness does not simply proxy for bank size. Lenders with high outdegree centrality have a larger network of lenders that participate in their deals and can credibly convey information they extract about borrowers to other investors.

Loan syndication networks may reduce the cost of debt by facilitating the transmission of information across lenders and reducing information asymmetries between lenders and borrowers (Marquez (2002)). If this hypothesis is true, the effect of lender interconnectedness on the cost of debt should be economically larger for firms that exhibit higher levels of information asymmetry. Consistent with this hypothesis, I find that an exogenous increase in outdegree centrality leads to 15-17 basis points reduction in firms' cost of borrowing on average, but only for private or unrated firms. The results are also more pronounced for well-performing firms, consistent with the view that well-connected lenders improve loan terms for high quality and credit constrained borrowers. The results are qualitatively similar when I also focus on a matched sample of firms.

I also investigate whether well-connected lenders affect other dimensions of loan contract terms, such as loan maturity, package amount, or the number and the strictness of financial covenants. I find that well-connected lenders are more likely to

offer extended loan maturities, and are less likely to demand collateral. However, I do not find evidence that interconnectedness affects loan amount, or the strictness of financial covenants in new loans.

This paper is related to a number of different studies. The findings are consistent with theoretical and empirical studies that focus on the organizational structure of underwriting syndicates and its effect of loan contracts. Specifically, theory suggests that relationships between underwriters can mitigate syndicate moral hazard, such as free-riding in screening and monitoring (Holmstrom (1982), Pichler and Wilhelm (2001)). The existence of information asymmetry between lenders implies that standard industrial organizational results may not apply for the loan market (Dell'Ariccia (2001), Marquez (2002)). This paper contributes to the literature by providing empirical support to the hypothesis that well-connected underwriters can convey information about loan value to other investors through the loan syndication process and affect the pricing of loan contract terms. Despite the significant reduction in the number of banks that are active in the syndicated loan market, I do not to find evidence that lenders collude or extract higher rents due to market power.

The results complement several empirical studies that investigate the role of investor networks. Grullon et al. (2014) find that investment banking networks affect trading behavior. Using measures of network centrality, Hochberg et al. (2007) find that VC networks affect fund performance, and Bajo et al. (2016) show that underwriter networks affect IPO characteristics. Houston et al. (2015) find that connections through lenders' boards facilitates information flows, and Cai et al. (2011) show that lender connectedness increases systemic risk. This paper also relates

to Barth et al. (2009), who find that information sharing between lenders reduces information asymmetry and improves the efficiency of lending decisions. Finally, the findings in this paper also complement the work of Sufi (2007) and Ivashina (2009), who show that information asymmetry between lenders affects the structure of loan syndicates and has a significant impact on loan prices.[3]

The rest of the paper proceeds as follows. Section I describes the dataset and the main variables of the empirical methodology. Section II presents the empirical results, and section III concludes.

2.2 Data and Empirical Strategy

2.2.1 Data and summary statistics

I collect data for all loans syndicated in the U.S. between 1992 and 2014 from Loan Pricing Corporation's (LPC) Dealscan database. Dealscan contains detailed loan contract information, such as the loan spread, maturity, amount, and financial covenants. I merge loan information from Dealscan with accounting information from Compustat's quarterly fundamental file using the link by Chava and Roberts (2008). After excluding financial companies, my final sample consists of 4,457 firms and 32,722 unique loan facilities. Firms with access to the syndicated loan market are large, with a median firm size of $1.5 billion (median firm in Compustat is about

[3]Focusing on municipal bonds, corporate bonds, and IPO/SEO underwriting, many studies highlight the importance of information asymmetry due to geographical distance or past relationships with a firm on the choice and the structure of underwriting syndicates (see Lee and Mullineaux (2004), Corwin and Schultz (2005), Yasuda (2005), Ljungqvist et al. (2006), and Butler (2008).

$150 million). Syndicated loans are large, the average loan amount is $450 million, and require multiple banks to participate to underwrite a deal. With an average maturity of 5 years, syndicated loans have shorter maturities than bonds and are commonly secured with collateral.

Dealscan provides the name of the lender, often using regional branch names, or the name of a subsidiary. I combine regional branches and subsidiaries operating with a different brand name under a common parent company.[4] I exclude from the sample lenders that have been the lead arranger for fewer than 20 loans throughout the whole sample period. This filter does not affect considerably the size of the sample as the top 100 lenders originate more than 80% of loans in Dealscan. The final dataset contains 158 lenders, including both depository and non-depository institutions, with an average of $960 billions in assets.

2.2.2 Bank consolidations

Mergers are important events for firms and have a large impact on their organizational and capital structure. Mergers between large firms can also cause significant changes in the structure of their industries (Gabaix (2011)). In the banking industry, deregulation has led to significant M&A activity between banks and changed the shape of the financial services industry (Berger et al. (1999)). Figure 2.1 shows the total number and the average size of lenders that originate loans and participate in the syndicated loan market. It is evident that the number of lenders has declined since 1990, and

[4] I am particularly thankful to Justin Murfin for his help linking regional branches at the parent level.

their average size has increased. The reduction in the number of banks participating in the syndicated loan market started before the repeal of the Glass Steagall Act and continued after the great recession.

The consolidation in the banking industry has affected the syndicated loan market and as a result a small number of large lenders dominate the market. The propensity of a few large banks to co-syndicate loans has increased considerably, creating a small number of highly interconnected lenders. Because bank mergers cause significant changes in banking industry, I use these large events as a source of variation in bank connectedness to investigate its effect on debt contracting.

I first identify all mergers between lenders in the sample. I use SDC Plantinum to track all mergers between lenders and search their individual histories to confirm the effective date of the merger. Dealscan does not provide identifiers for lenders at the parent level, so I hand-match lenders from Dealscan with SDC Plantinum. To ensure that these mergers have a significant impact on the new lender, I keep only bank mergers that involve a change in control. From this procedure I identify 79 mergers between U.S. and international lenders during the sample period.[5]

2.2.3 Bank interconnectedness

To measure the interconnectedness of a lender I use tools from network analysis. Social network analysis is helpful in studying different types of relationships that lenders

[5]I am very thankful to Alvaro Taboada for his help identifying bank mergers that involved a change in control.

develop through frequent interactions with each other during the loan syndication process.

For every lender, I calculate three different measures of network centrality to capture its interconnectedness: degree centrality, outdegree centrality, and indegree centrality. Degree centrality counts the number of ties an agent has with every member in the network. To calculate degree centrality, I count the number of different lenders that a bank has co-syndicated with during the past four quarters and divide this number by the total number of lenders. For instance, assume there are four banks in the network, bank A, bank B, bank C, and bank D. In this example, every bank has the potential to work with three different banks ($C_{max} = 3$). If bank A co-syndicated loans with bank B and C over the last year, then its degree centrality is 2 ($c_i = 2$), and its normalized measure of degree centrality is 2/3 or 0.66 ($=\frac{c_i}{C_{max}}$).

Moreover, syndicated loan networks are directed networks, creating significant heterogeneity in the types of relationships that lenders develop with each other. Specifically, a bank can be the lead underwriter for a syndicated loan and can create a relationship with other lenders by inviting them to participate in its own deals. But a bank can also develop a relationship with other lenders by joining as a participating agent in their loan deals. Unlike market share, centrality measures can capture different dimensions of the relationships embedded in different roles that banks have in loan deals.

To capture the heterogeneity in the relationships between lenders, I calculate for every bank its outdegree and indegree centrality. To measure outdegree centrality for a lender, I count the number of different banks that have agreed to join in its deals as

participants ($c_i(lead)$), and normalize by the total number of banks in the network ($\frac{c_i(lead)}{C_{max}}$). This measure captures the bank's confidence in underwriting new loans and attracting investors in its own deals. In the previous example, assume that bank A has co-syndicated with banks B and C in two different deals, but only with bank B it has worked as a lead arranger. In this case, the outdegree centrality of bank A is 1/3 or 0.33.

I also calculate a bank's indegree centrality to capture a bank's ability to access information about borrowers' they otherwise would not have access to. To calculate indegree centrality, I count the total number of banks that bank A has a relationship with by joining their deals as a participant ($c_i(part.)$) and normalize by the total number of banks in the network ($\frac{c_i(part.)}{C_{max}}$). In the previous example, if bank A joins a loan syndicate arranged by bank C, its normalized indegree centrality measure will be 1/3 or 0.33.

Figure 2.2 shows the time-series average of degree and outdegree centrality of banks in the sample based on relationships developed through loan syndicates over a rolling four-quarter period. The figure confirms that bank interconnectedness, measured by average degree centrality, has increased alongside the propensity of banks to co-syndicate loans. From this figure the increase in outdegree centrality stands out, especially after the most recent financial crisis. The large increase in outdegree centrality is driven by the consolidation between some of the largest lenders in the industry in 2008-2009 such as Bank of America and Merrill Lynch, Wells Fargo and Wachovia, and JP Morgan and Bear Stearns.

Along with the measures of lender centrality, I also calculate the market share of the average lender in the sample. To calculate the market share for a lender, I count the total dollar amount of its loans and divide it by the total dollar amount of loans issued by all banks during the past four quarters. The challenge in calculating market share is that multiple banks contribute funds to underwrite a loan deal, and it is common for banks—even for the lead arranger— to contribute 20% of the total amount of the loan or less. Therefore, when I measure the total market share for a lender, I multiply the total loan amount by the percentage share that the lender contributes to this loan. However, Dealscan does not always report the share that lenders contribute to a loan; for these cases I assume that all participating banks contribute equally to the loan. This simplifying assumption eases the construction of the variable *market share*, but underestimates the share that large lead arrangers have in the market.

I find that throughout the sample period the median lender has market share of approximately 5.4%. As we would expect, lenders with a large market share are also more interconnected, with correlations ranging from 0.38 to 0.67 (see Table 2.2). In Table 2.1, the average measure of degree centrality is 0.46, which implies that the average bank is connected to 46% of the other banks in the network. Average indegree centrality is also high (45%), which suggests that banks frequently join syndicates originated by other banks. As we would expect, outdegree centrality is considerably lower; the median bank in the sample invites in its own deals approximately one out of four banks in the sample.

2.3 Empirical Results

2.3.1 Lender networks and loan pricing

To study the effects of lender interconnectedness on borrowers' cost of debt, I begin by regressing loan spreads on lead arrangers' network centrality measured by degree, outdegree, and indegree centrality. Because the effects of lender interconnectedness on the cost of borrowing may be non-linear, I create three groups of lender centrality (High, Medium, and Low) based on the yearly distribution of each measure. The baseline specification I use to explain the effect of lender interconnectedness on loan spreads is the following:

$$\text{Loan Spread}_{i,t,b} = \alpha_i + \alpha_t + \beta X_{i,b,t} + \gamma(\text{Lender Centrality High})_{b,t} + \epsilon_{i,t}. \quad (2.1)$$

In the regression above, i indexes borrowers receiving a loan from lender b. The set of control variables $X_{i,b,t}$ capture a set of time varying borrower characteristics (assets, market-to-book, leverage, and Altman Z-score), loan characteristics (maturity, amount, secured by collateral, and number of banks that participate in the syndicate), and lender characteristics (bank assets, bank deposits). To control for unobservable and fixed-over-time borrower characteristics, the regressions include borrower fixed effects. The regression also includes year and rating fixed effects.

Table 2.3 shows the results of the baseline specification. The results suggest that highly interconnected lenders offer, on average, lower interest rates to borrowers. In column (1), the cost of debt for a firm borrowing from a lender with high degree

centrality is approximately 12 basis points lower compared to a lender with low degree centrality. Because more interconnected lenders are also larger in size, I include a control variable for bank size to mitigate the possibility that large lenders make more competitive interest rates. The results remain quantitatively identical when I control for bank deposits as well (not tabulated), but I do not include deposits in the baseline regressions because some lenders do not carry deposits and drop from the sample.

From Table 2.3, there are two results that stand out. First, among the different measures of lender interconnectedness outdegree centrality appears to have the largest effect on the cost of debt. Specifically, a lender with high outdegree centrality offers borrowers on average 15.6 basis point lower debt spreads. This result suggests that banks make loans with lower interest rates if they are confident in their ability to underwrite a loan deal by attracting other investors to participate in its loan deals.

I find evidence that a large market share also leads to lower interest rates. The second result that stands our from Table 2.3, however, is that when I include both outdegree centrality and market share in the regression in Column (5), the economic effects of market share on interest rates drops considerably. This is not the case for outdegree centrality, which remains economically large and statistically significant. Taken together, these results suggest that lender interconnectedness explains a different part of the variation of borrowers' cost of debt.

2.3.2 Lender interconnectedness and loan pricing - Evidence from bank mergers

A potential problem in studying the effects of lender interconnectedness on debt contracting is the endogenous matching between well-connected banks and low-risk borrowers. Specifically, if highly-interconnected lenders lend to borrowers with low credit risk, the effect of interconnectedness on cost of borrowing could be due to self-selection bias.

To address this type of endogeneity, I exploit shocks in the structure of bank networks after bank mergers. Bank mergers serve a dual role in identifying the effect of lender interconnectedness on the cost of debt. First, bank provide significant variation in bank size and centrality. Specifically, the average increase in banks' assets after a merger is 21%, and the increase in degree centrality is 17%. Second, and more importantly, I identify firms that had a relationship with the acquired bank and extend this relationship with the new bank after the merger. The merger, therefore, provides variation in lender connectedness that is exogenous to firm characteristics and firm-bank matching. In other words, by focusing on the sample of firms that have an existing relationship with one of the merging banks, the match between the firm and a lender with high centrality is plausibly random.

To identify the effects of an exogenous increase in lender interconnectedness after a bank merger on the cost of borrowing, I use the following regression model:

$$\begin{aligned}\text{Loan Spread}_{i,t,b} = \alpha_i + \alpha_t + \beta X_{i,b,t} + \\ + \gamma_1(\text{Post Merger}) + \gamma_2 \Delta(\text{Lender Centrality})\text{-High} \\ + \gamma_3(\text{Post Merger}) \times \Delta(\text{Lender Centrality})\text{-High} + \epsilon_{i,t}.\end{aligned} \tag{2.2}$$

In regression (2.2), *Post-merger* is a dummy variable that is equal to one if the loan origination date is after the date the lead arranger merged with the previous lender of the firm. I also impose the restriction that the firm must have had at least two loans in the past five years with the acquired bank to ensure that the borrower had a relationship with that bank. Coefficient γ_1 captures the change in the cost of debt for borrowers after the current lender acquired their previous lender.

Δ*(Lender Centrality) High* is an indicator variable that takes the value of one if the firm borrows from a lender with a large change in centrality. The change in lender centrality is large if it belongs in the top tercile of changes of the yearly distribution of network centrality (degree, outdegree, and indegree). These network measures capture the average effect of a large increase in lead arranger interconnectedness on the cost of debt.

Table 2.4 presents the regression results. The average effect of borrowing from a highly interconnected lender varies between 9 and 15 basis points. The coefficient of interest is γ_3 and identifies the change in the cost of debt after a merger, for a borrower whose lead arranger interconnectedness changed from low to high. The

interaction term between *Post-merger* and Δ*(Lender Centrality) High*, therefore, provides a causal estimate of an increase in network interconnectedness on the cost of debt.

One main result that stands out from Table 2.4 is that outdegree centrality is the only measure of interconnectedness that remains statistically significant and economically large. I find that loan spreads decline by an average of 8 basis points after a merger that significantly increases a lenders' outdegree centrality. The results suggest that outdegree centrality increases lenders' ability to find other banks to participate in its deals and can underwrite loans with improved loan terms.

Despite the decline in the number of banks that are active in the syndicated loan market and the increase in their market share, I do not find evidence that highly interconnected lenders extract rents from their borrowers. On the contrary, the evidence in this paper suggests that highly interconnected banks lower borrowers' cost of debt.

2.3.3 Lender networks, loan pricing, and information asymmetry

Theory suggests that interconnected underwriters, by collaborating with other lenders, improve their ability to screen and monitor borrowers (Esty (2001), Marquez (2002)). Bajo et al. (2016) find that well-connected IPO underwriters can better extract and disseminate information to investors, and Barth et al. (2009) find that information sharing across lenders improves the efficiency of lending decisions.

If well-connected lenders reduce borrowing costs by mitigating information asymmetries between the borrower and other lenders, the reduction in the cost of debt

should be higher for firms that exhibit higher levels of information asymmetry. To test this hypothesis, I investigate whether the marginal effect from a change in lender interconnectedness is different for private or unrated firms. These firms exhibit higher levels of information asymmetry to investors, so I hypothesize that an increase in interconnectedness should lead to larger decrease in the cost of debt for private or unrated firms.

To test this hypothesis, I split the sample between firms with high and low information asymmetry. I present the estimates of a regression specification as in equation (2.2) in Table 2.5. The top panel regressions use a subsample of firms with high information asymmetry—private firms (column 1) and unrated firms (column 2). The bottom panel focuses only on the subsample of firms with low information asymmetry—public firms (column 1) and rated firms (column 2).

The results in Panel A of Table 2.5 suggest that an exogenous increase in lender outdegree centrality from low to high leads approximately to a 15-17 basis point reduction in the cost of debt, but only for private and unrated borrowers. This effect is economically larger than the full sample results in Table 2.4. On the other hand, an exogenous switch to a lender with high outdegree centrality does not affect the cost of debt for borrowers that present lower levels of information asymmetry (Panel B).

To further corroborate the hypothesis that highly connected lenders mitigate information asymmetries without affecting the riskiness of their loan portfolio, I also investigate the change in the cost of debt for firms that are close to distress. A firm is closer to distress if it is at the top tercile of the yearly distribution of Z-score

in the sample. Columns (1) and (2) in Table 2.6 show the regression estimates of an exogenous increase in lender outdegree centrality on loan spreads for low- and high-risk firms, respectively. Interestingly, outdegree centrality leads to a statistically significant reduction in the cost of debt only for high quality firms, which suggests that the reduction in interest rates is not due to a race-to-the-bottom effect.

2.3.4 Lender networks and non-pricing contract terms

It is also possible that except for the reduction in the cost of borrowing, lender interconnectedness also affect non-pricing contract terms, such as loan maturity, package amount, or covenant strictness. Previous literature shows that prior lending relationships and information asymmetry affects the strictness of loan covenants (Hollander and Verriest (2016), Prilmeier (2016)), or the maturity and amount of new contracts. The regressions control for loan characteristics, but in this section I test directly whether lender interconnectedness affects non-pricing loan contract terms.

I use the regression model in specification (2.2) to identify exogenous variation in network centrality of a borrower's lead arranger, and test the effects of lender interconnectedness on loan maturity, loan amount, and collateral requirements. The results in Table 2.7 suggest that an increase in lender interconnectedness after a merger increases loan maturity approximately by 4% (column 1). Well-connected lenders are also less likely to require firms to use their assets as collateral (see column 3). Although these changes are not economically large, the results are consistent with the previous findings in this paper.

I also investigate whether lender interconnectedness affects the number of financial covenants, or the strictness of loan contract terms. Table 2.8 presents the regression results. An exogenous increase in lender interconnectedness does not affect the number or the strictness of loan covenants that lenders impose on borrowers. Taken together, the evidence suggests that well-connected lenders do not exploit their position in the network to extract rents from their borrowers, and alternatively improve the pricing and non-pricing terms of new loan issues.

2.3.5 Robustness - Matching estimators

Well-connected lenders are different from lenders that are not well-connected in the network of lenders that frequently underwrite and participate in syndicated loans. In Panel A of Table 2.9 I test for covariate balance between lenders that have high outdegree centrality versus those that have low outdegree centrality. Well-connected banks are considerably larger, they have more deposits, and they tend to make loans to larger firms with higher credit quality.

These differences could confound the estimated effect of high lender centrality on loan contract terms. To address these concerns, I focus on a matched sample of treated and control firms. A treated observation is a firm-loan unit originated by a bank that has a previous relationship with the firm, and the its outdegree centrality increases from low to high after the merger. I match treated and control firms on the basis of their size, leverage, market-to-book ratio, and riskiness (Altman's Z-score), as well as lender size and bank deposits. The matching is produces a set of treated and control firm-loan observations with balanced characteristics (see Table 2.9). Finding a

good match for large and highly interconnected banks is possible because the sample includes numerous international banks, such as Credit Acrigole or UBS, that are large in size but not as highly connected in the U.S. syndicated loan market as domestic banks.

In Panel B, I use the Abadie and Imbens (2006) nearest-neighbor, bias-corrected, and heteroskedasticity-consistent estimator to estimate the average treatment effect on the treated (ATT). The estimated treatment effect of borrowing from a well-connected lead arranger is approximately -16 basis point. This estimate is consistent with the full sample OLS estimator, and economically identical with the effect of outdegree centrality on borrowing costs of firms with high levels information asymmetry.

2.4 Conclusion

I find that a small number of interconnected banks dominates the syndicated loan market. In this market, measures of market concentration are irrelevant because banks share information and resources with syndicate members. I test whether lender interconnectedness is good for borrowers resulting in efficiency gains, or bad resulting on rent extraction through market power. Using bank consolidations to assign borrowers exogenously to a new lender, I find that an increase in outdegree centrality leads to a significant reduction in firms' cost of debt. The effect is economically larger for private and unrated borrowers, consistent with the hypothesis that syndication networks reduce the cost of debt and other loan contract terms by facilitating the transmission of information across lenders.

2.5 Tables & Figures Appendix

2.5.1 Tables

APPENDIX: Definition of Variables
This table provides details for the variables used throughout the paper. The sample period is 1994 through 2015. Accounting data are from Compustat's quarterly file. Loan information is from Dealscan. Bank information is from Compustat NA Bank, Compustat Global, and Bankscope. I winsorize all variables at the 1st and 99th percentile.

Variable Names	Description
	Firm Characteristics
Assets	Book Assets
Book Leverage	Total Debt / Book Assets
ROA	EBITDA / Book Assets
Market-to-Book	(Market Equity + Total Debt + Preferred Stock liquidating value - Deferred Taxes and Investment Tax Credits) / Book Assets
Z-Score	3.3×Pre-tax Income/Assets + 0.999×Sales/Assets + 1.4 ×Retained Earnings/Assets + 1.2 × (Current Assets - Current Liabilities)/Assets +0.6×Mkt Equity/Total Liabilities
	Loan Characteristics
Loan Spread	The All-in-Drawn Spread from Dealscan, for each dollar borrower draw, excluding fees.
Loan Amount	The sum of all amounts in a certain loan package.
Loan Maturity	The number of months between the earliest loan origination date and the latest maturity date in a certain loan package.
Loan Participants	The total number of participating banks (non lead-arrangers) in a certain loan package.
Covenant Number	Total number of financial covenants in the loan contract.

Contract Strictness	Indicates the probability that the firm will violate at least one of its covenants in the next quarter (following methodology from Murfin (2012)).
	Bank Network Characteristics
Market Share	The percent of total loan volume (rolling four-quarter) originated by the lender.
Degree Centrality	The percent of all lenders that a lender has co-syndicated with over the past year (rolling four quarter).
Outdegree Centrality	The percent of all lenders that a lender, while acting as a lead arranger, has invited in loan syndications during the past year (rolling four quarters).
Indegree Centrality	The percent of all lenders that a lender, has been invited (as a non-lead arranger) in loan syndications during the past year (rolling four quarters).

Table 2.1: **Summary Statistics**
This table presents summary statistics of firm, loan, governance, and bank characteristics. Accounting information is from Compustat. Loan information is from LPC Dealscan. Bank information is from Compustat NA Bank, Compustat Global, and Bankscope. Governance variable are from various sources cited in the Appendix. All variables are described in detail in the Appendix.

Variable	**N**	**Mean**	**SD**	**10%**	**50%**	**90%**
			Firm Characteristics			
Total Assets(m.)	32722	6510.205	12990.5	119.145	1563.85	17428
ROA	32060	0.03	0.03	0.01	0.03	0.06
Market Value/Book Value	32722	1.390	.940	.607	1.127	2.445
Has SP rating	32722	.547	.497	0	1	1
Altman-Z	32722	3.844	4.419	-.252	3.606	8.639
Book Leverage	32722	.343	.243	.056	.303	.658
			Loan Variables			
Spread	32722	215.473	132.289	52.5	200	375
Amount	32722	453.781	650.394	17.922	200	1225
Maturity	32722	48.849	22.014	12	60	72
Secured	32722	.497	.500	0	0	1
Participants (#)	32722	5.372	4.058	1	4	11
Cov.Strictness	12367	20.799	17.328	.115	18.558	45.038
			Bank Characteristics			
Bank Assets(b.)	32722	967.537	821.679	91.122	686.36	2289.24
Bank Deposits(b.)	31548	472.887	374.290	49.994	356.664	1079.177
			Bank Network Characteristics			
Market Share	32712	0.056	0.035	0.012	0.054	0.100
Degree Centrality	32722	0.462	0.061	0.422	0.476	0.505
Indegree Centrality	32722	0.451	0.060	0.412	0.462	0.496
Outdegree Centrality	32722	0.282	0.116	0.131	0.242	0.455

Table 2.2: **Pairwise Correlations**
This table presents pairwise correlations of lender network measures and market share. Degree centrality is the percent of all lenders that a lender has co-syndicated with over the past year (rolling four quarter). Indegree centrality is the percent of all lenders that a lender, has been invited (as a non-lead arranger) in loan syndications during the past year (rolling four quarters). Outdegree centrality is the percent of all lenders that a lender, while acting as a lead arranger, has invited in loan syndications during the past year (rolling four quarters). Market share is the percent of total loan volume (rolling four-quarter) originated by the lender. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Centrality Degree	Centrality Indegree	Centrality Outdegree	Market Share
Degree Centrality	1			
Indegree Centrality	0.982***	1		
Outdegree Centrality	0.791***	0.737***	1	
Market Share	0.431***	0.380***	0.679***	1

Table 2.3: **Lender networks and loan prices**
The dependent variable in all regressions is the loan spread measured in basis points. In regressions (1)-(5), the independent variable of interest is an indicator for a loan originated by a lead arranger at the top tercile of the yearly distribution of: (1) the percent of all lenders that the lead arranger has co-syndicated with over the past year (*Degree centrality*); (2) the percent of all lenders that the lead arranger has invited in its loan syndications during the past year (*Outdegree centrality*); (3), the percent of all lenders that have invited the lead arranger in their loan syndications during the past year (*Indegree centrality*); And (4) the percent of total loan volume originated by that lead arranger (*Market Share High*). The regressions include firm, bank, and loan controls, and also year, rating, and borrower fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm, lender, and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Spread	(2) Spread	(3) Spread	(4) Spread	(5) Spread
Degree (High)	-11.9***				
	(3.0)				
Outdegree (High)		-15.6***			-13.4***
		(3.9)			(3.8)
Indegree (High)			-10.4***		
			(2.2)		
Market Share (High)				-10.3***	-4.6*
				(3.0)	(2.2)
Book Leverage	51.4***	51.3***	52.3***	51.2***	51.4***
	(10.8)	(10.7)	(10.8)	(10.8)	(10.7)
Market Value/Book Value	-11.8***	-11.7***	-11.7***	-11.8***	-11.8***
	(1.5)	(1.6)	(1.5)	(1.6)	(1.6)
Altman-Z	-3.1***	-3.1***	-3.0***	-3.1***	-3.1***
	(0.4)	(0.4)	(0.5)	(0.4)	(0.4)
Ln(Assets)	-18.3***	-17.6***	-18.2***	-18.1***	-17.8***
	(3.5)	(3.4)	(3.5)	(3.4)	(3.4)
Ln(Bank Assets)	-1.2	0.8	-2.1	-0.3	1.4
	(1.8)	(2.0)	(1.9)	(2.0)	(2.1)
Observations	31681	31681	31662	31670	31670
Adjusted R^2	0.667	0.667	0.666	0.666	0.667
Firm-Bank Controls	Yes	Yes	Yes	Yes	Yes
Year-Rating FEs	Yes	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes	Yes

Table 2.4: **Variation in interconnectedness due bank M&A activity**
The dependent variable in all regressions is the loan spread in basis points. *Post-merger* is a dummy variable that is equal to one if the loan origination date is after the date the lead arranger merged with the previous lender of the firm. The independent variable of interest is the interaction between *Post Merger* and an indicator whether the lead arranger is at the top tercile of the yearly distribution of: *Market Share High* (column 1), *Degree centrality* (column 2), *Outdegree centrality* (column 3), and *Indegree centrality* (column 4). Section 2.2.3 provides details for the construction of these variables. The regressions also include the following loan controls: maturity, amount, participants, collateral, and spread. All regressions include firm, year, and rating fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm, lender, and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Spread	(2) Spread	(3) Spread	(4) Spread
Post Merger	-5.9	-5.3	-6.5*	-3.3
	(3.5)	(3.4)	(3.4)	(3.1)
Degree (High)	-9.3***			
	(3.0)			
Outdegree (High)		-15.3***		
		(4.4)		
Indegree (High)			-9.4***	
			(2.5)	
Market Share (High)				-7.8**
				(2.9)
Post Merger(X)High-Degree	-6.1			
	(4.0)			
Post Merger(X)High-Outdegree		-8.1**		
		(2.9)		
Post Merger(X)High-Indegree			-6.2	
			(4.4)	
Post Merger(X)High-Mkt.Share				-3.8
				(3.4)
Observations	14766	14766	14764	14761
Adjusted R^2	0.658	0.659	0.658	0.683
Firm-Bank Controls	Yes	Yes	Yes	Yes
Year-Rating FEs	Yes	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes	Yes

Table 2.5: **Bank networks and information asymmetry**
This table shows the regressions of loan spreads on changes of bank industry measures after bank M&A activity. Firms exhibits high (low) information asymmetry if it is private (public) or it is not rated by S&P (rated). *Post-merger* is a dummy variable that is equal to one if the loan origination date is after the date the lead arranger merged with the previous lender of the firm. The independent variable of interest is the interaction between *Post Merger* and an indicator whether the lead arranger is at the top tercile of the yearly distribution of *Outdegree centrality* (=the percent of all lenders that the lead arranger has invited in its loan syndications during the past year). The regressions include firm, bank, and loan controls (more details in the Appendix), as well as rating, year, and borrower fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm, lender, and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

(A: High Information Asymmetry)

	(1) Spread *(Private Firms)*	(2) Spread *(Unrated Firms)*
Post Merger(X)High-Outdegree	-17.31**	-15.15***
	(6.41)	(4.28)
Observations	3376	4295
Adjusted R^2	0.626	0.615
Firm-Bank Controls	Yes	Yes
Year-Rating FEs	Yes	Yes
Firm FEs	Yes	Yes

(B: Low Information Asymmetry)

	(1) Spread *(Public Firms)*	(2) Spread *(Rated Firms)*
Post Merger(X)High-Outdegree	-6.09*	-3.07
	(3.51)	(3.07)
Observations	11366	10400
Adjusted R^2	0.686	0.710
Firm-Bank Controls	Yes	Yes
Year-Rating FEs	Yes	Yes
Firm FEs	Yes	Yes

Table 2.6: **Changes in lender interconnectedness and firm risk**
This table shows the regressions of loan spreads on changes of bank industry measures after bank M&A activity. The sample includes only observations for firms who had a relationship with a lender that got acquired by another bank. A firm is a high-risk (low-risk) firm if Altman's Z-score is below (above) the yearly median. The dependent variable in all regressions is debt spreads measured in basis points. *Post-merger* is a dummy variable that is equal to one if the loan origination date is after the date the lead arranger merged with the previous lender of the firm. The independent variable of interest is the interaction between *Post Merger* and an indicator whether the lead arranger is at the top tercile of the yearly distribution of *Outdegree centrality*. The regressions include firm, bank, and loan controls (more details in the Appendix), as well as rating, year, and borrower fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm, lender, and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Spread *(Low Risk Firms)*	(2) Spread *(High Risk Firms)*
Post Merger	-3.82	-3.69
	(4.34)	(3.09)
Outdegree (High)	-15.92***	-15.57***
	(4.58)	(4.65)
Post Merger(X)High-Outdegree	-6.64***	-9.34
	(2.29)	(10.32)
Observations	10292	4376
Adjusted R^2	0.695	0.588
Firm-Bank Controls	Yes	Yes
Year-Rating FEs	Yes	Yes
Firm FEs	Yes	Yes

Table 2.7: **Bank networks and debt contract terms**
In columns (1)-(3) the dependent variable is, respectively: (1) the maturity of the loan measured in months, (2) the loan amount, and (3) secured is an indicator variable that takes the value of 1 if the loan issue is secured with collateral. *Post-merger* is a dummy variable that is equal to one if the loan origination date is after the date the lead arranger merged with the previous lender of the firm. The independent variable of interest is the interaction between *Post Merger* and an indicator whether the lead arranger is at the top tercile of the yearly distribution of *Outdegree centrality*. The regressions include firm, bank, and loan controls (more details in the Appendix), as well as rating, year, and borrower fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm, lender, and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	Ln(Maturity)	Ln(Amount)	Secured
Post Merger	0.0280	0.102**	-0.0229
	(0.0196)	(0.0365)	(0.0165)
Outdegree (High)	-0.0364*	0.0867***	0.0108
	(0.0201)	(0.0288)	(0.0114)
Post Merger(X)High-Outdegree	0.0388**	-0.0312	-0.0183*
	(0.0169)	(0.0317)	(0.00934)
Book Leverage	-0.0644	0.0969	0.0571
	(0.0745)	(0.117)	(0.0738)
Market Value/Book Value	-0.0299	0.0939***	-0.0301*
	(0.0189)	(0.0286)	(0.0152)
Altman-Z	0.0151**	0.0124	0.0000187
	(0.00575)	(0.00947)	(0.00370)
Ln(Assets)	-0.0323	0.454***	-0.104***
	(0.0226)	(0.0372)	(0.0183)
Ln(Amount)	0.0816***		-0.000917
	(0.0136)		(0.00788)
Ln(Maturity)		0.174***	0.0668***
		(0.0307)	(0.00687)
Observations	15791	15791	15791
Adjusted R^2	0.330	0.681	0.510
Firm-Bank Controls	Yes	Yes	Yes
Year-Rating FEs	Yes	Yes	Yes
Firm FEs	Yes	Yes	Yes

Table 2.8: **Bank networks and debt contract terms**
In column (1), the dependent variable is the strictness of loan covenants and represents the probability that the firm will violate at least one covenant over the next quarter (see Murfin (2012)). In column (2), the dependent variable is the number of covenants in the contract. The independent variable of interest is the interaction of *Post Merger* and an indicator whether the lead arranger is at the top tercile of the yearly distribution of *Outdegree centrality*. The regressions include firm, bank, and loan controls (more details in the Appendix), as well as rating, year, and borrower fixed effects. Standard errors are reported below each regression coefficient, and are clustered at the firm, lender, and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Cov.Strictness	(2) Covenants (#)
Post Merger	-1.320	-0.0671
	(0.937)	(0.0661)
Outdegree (High)	-0.685	-0.0724*
	(0.491)	(0.0372)
Post Merger(X)High-Outdegree	0.957	-0.00742
	(0.939)	(0.0564)
Book Leverage	46.57***	0.377
	(5.892)	(0.269)
Market Value/Book Value	-4.393***	-0.125***
	(1.029)	(0.0403)
Altman-Z	0.293	0.0298*
	(0.325)	(0.0144)
Ln(Assets)	-0.0160	-0.105**
	(0.840)	(0.0491)
Ln(Maturity)	-0.185	0.0765**
	(0.372)	(0.0296)
Ln(Amount)	-0.854**	-0.0311
	(0.315)	(0.0183)
Observations	5340	5340
Adjusted R^2	0.627	0.676
Firm-Bank Controls	Yes	Yes
Year-Rating FEs	Yes	Yes
Firm FEs	Yes	Yes

Table 2.9: **Matching Estimator**
This table compares the effect of borrowing from a lender with high outdegree centrality and a lender with low outdegree centrality. In this sample, a treated observation is a loan of a firm whose prior lender got acquired by a lender and the merged bank has high outdegree centrality (top tercile). A control observation is a loan of a firm whose prior lender got acquired by a lender and the merged bank has low outdegree centrality (bottom two terciles). I calculate outdegree centrality based on the share of all banks that a lender, acting as a lead arranger, has invited in loan syndications during the past four quarters. I match treated and control firms on the basis of their size, leverage, market-to-book ratio, and riskiness (Altman's Z-score), as well as lender size (assets) and deposits. Panel A shows the imbalance between treated and control observations for the full sample and for the matched sample. In Panel B, the matching estimator is the Abadie and Imbens (2006) nearest-neighbor, bias-corrected, and heteroskedasticity consistent estimator for the average treatment effect on the treated (ATT). I report robust standard errors in parethenses. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Treated = Post merger transfer to lender with high outdegree centrality
Control = Post merger transfer to lender with low outdegree centrality

A: Covariate Balance

	Outdegree (High)	Outdegree (Low)	Difference (full sample)	Difference (matched sample)
Ln(Bank Assets)	13.69	12.65	1.04***	0.05*
Bank Deposits	6.12	5.12	0.97***	0.00
Ln(Firm Assets)	7.58	6.93	0.65***	0.01*
Leverage	0.32	0.31	0.01***	0.00
Mkt-Book	1.44	1.44	0.00	-0.01
Altman-Z	4.05	3.52	0.53***	0.02

B: Matching Estimators

	Difference in debt spread (bp) (Treated-Control)
Abadie-Imbens estimator	-16.39***
(Avg. treatment effect on treated)	(3.50)

2.5.2 Figures

Figure 2.1: Number of banks and average bank size.
This figure shows the total number of banks (straight line) and the average size (dashed line) for lenders participating and arranging loans in the syndicated loan market from 1994 to 2015. Banks or finance companies without information on size of assets, or that have arranged fewer than 20 loans during the sample period are excluded. Lenders information is aggregated at the parent level. The first vertical line represents the date of repeal of Glass-Stegall Act, and the second vertical line represents the date of Lehman Brothers' collapse.

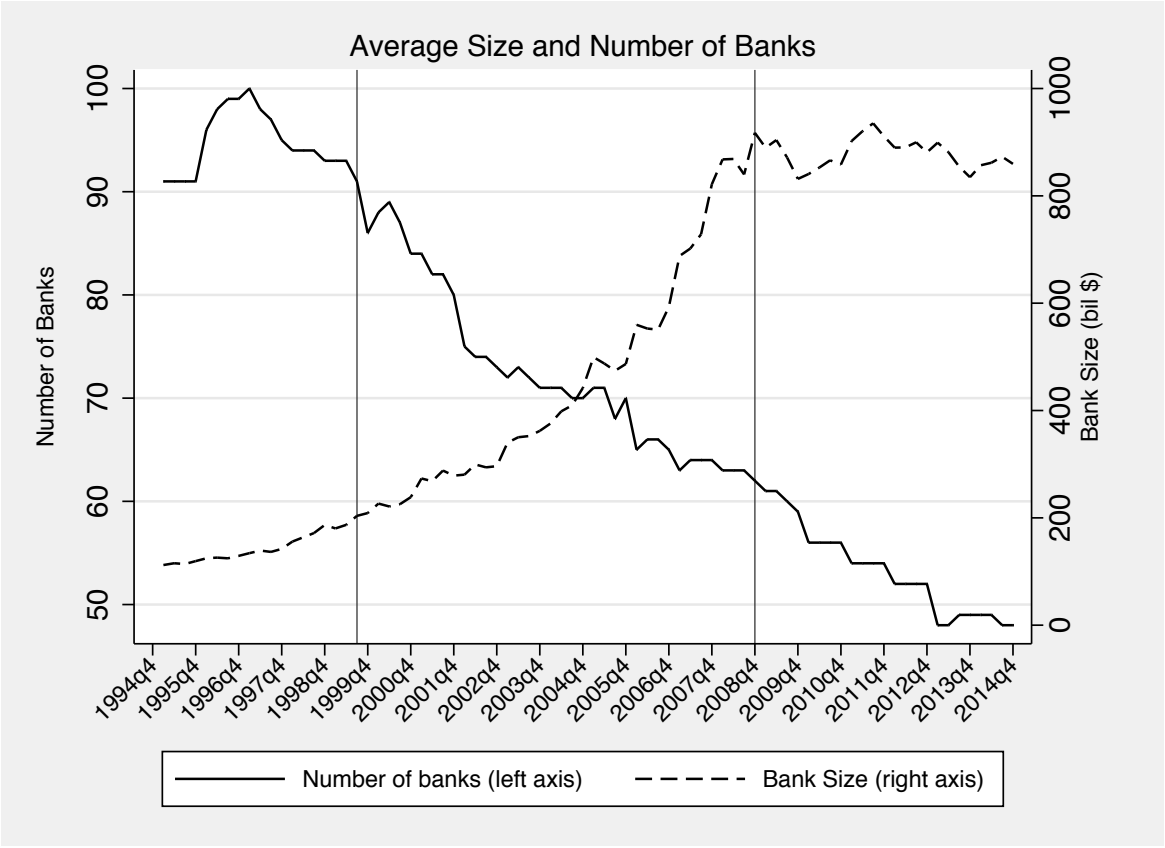
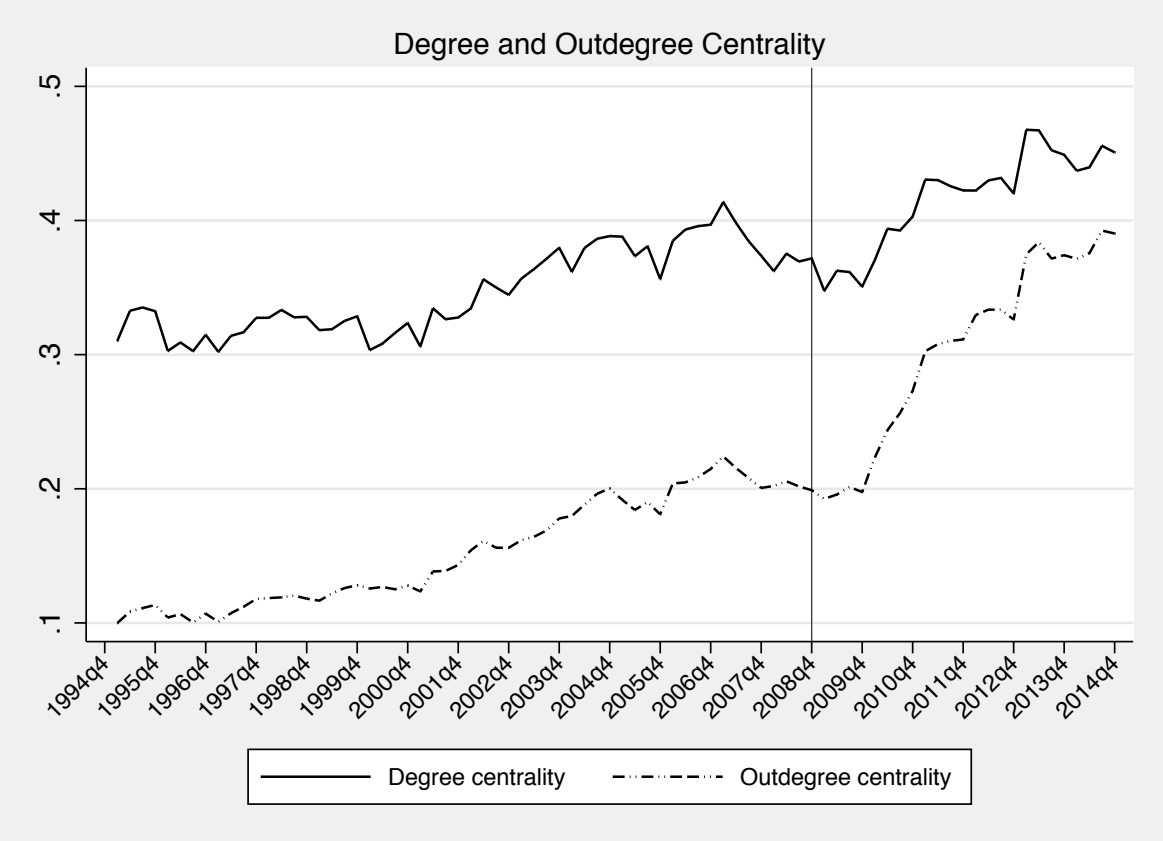


Figure 2.2: Degree and Outdegree Centrality
This figure shows the sample average degree measure (straight line) and outdegree measure (dashed line) of bank network centrality. The sample period is 1994 through 2015. Average degree centrality measures the percent of all banks that the average bank has co-syndicated with over the past year (rolling four quarter). Average outdegree centrality measures the percent of all banks that the average bank, while acting as a lead arranger, has invited in loan syndications during the past year. Lenders without information on assets, or lenders that have arranged fewer than 20 loans during the sample period, are excluded. Information for lenders is aggregated at the parent level. The vertical line represents the date of Lehman Brothers' collapse.



— Chapter 3 —

Creditor Rights and Debt Structure in the Zone of Insolvency

3.1 Introduction

How do firms determine their corporate debt structure? Firms issue secured or unsecured debt, senior or subordinated bonds, and provide different control rights and priorities to creditors (Diamond (1991), Boot and Thakor (1993), Park (2000), Rauh and Sufi (2010)). However, despite the significant heterogeneity in types of debt on firms' capital structure, we have little evidence about the factors that affect the structure of corporate debt. In this paper, I investigate whether creditor protection rights explain part of the heterogeneity in firms' debt structure.

Theory offers mixed predictions for the effects of creditor control rights on firms' debt structure. On one hand, increasing creditors' ability to enforce contracts should expand creditors' supply of secured debt and lower the cost of capital (Shleifer and Vishny (1986), Lopez-De-Silanes and La Porta (1997), Vig (2013)). On the other hand, allowing senior secured creditors to interfere with firm policies when they are near distress, may also lead to a reduction in firms' demand for secured debt to mitigate conflicts of interest between senior and subordinated stakeholders (Aghion et al. (1992), Diamond (1993), Ayotte and Morrison (2009)).

Disentangling the effects of creditor protection rights on debt structure faces two empirical challenges. First, corporate debt structure is determined by existing agency conflicts in a firm, and therefore, one needs exogenous change in creditor control to identify a causal relationship. Second, changes in creditor protection rights may affect simultaneously firms' demand for secured debt, but also creditors' supply of secured debt. If the supply side effect of creditor rights dominates, an increase in creditor protection should lead to an increase in the share of secured to total debt; if the demand side effect dominates, firms may choose to reduce their exposure to secured debt and the share of secured to total debt should decline.

To address the endogenous relationship between creditor control rights and debt structure, I use a 1991 Delaware court ruling as a source of exogenous variation in creditor protection rights. The Delaware court ruling resolved a legal case between Credit Lyonnais and Pathe Communications in favor of Credit Lyonnais, the senior secured creditor. The court ruling favored creditors and determined that in the vicinity of insolvency the board of directors owes its duty not only to shareholders but creditors alike. The court held in dictum: *"At least where a corporation is operating in the vicinity of insolvency, a board of directors is not merely the agent of the residue risk bearers, but owes its duties to the corporate enterprise."* The court decision, by acknowledging the management's duty to protect creditors' interest when the firm is close to bankruptcy, implied a significant change from the status quo and received extensive coverage from the media, as well as from business and law related journals.[1]

[1]Footnote 55 of the decision received special attention from legal companies: *"Such directors will recognize that in managing the business affairs of a solvent corporation in the vicinity of insolvency,*

In 2004, Delaware's court reversed 1991's court decision by limiting lenders' ability to sue corporate executives for a breach of fiduciary responsibility to senior secured creditors.

Using the 1991 Delaware court ruling as an exogenous increase in creditor control rights, I find that the share of secured to total debt of firms incorporated in Delaware (treated firms) increased by approximately 2.5%. This result is consistent with La Porta et al. (1998), who suggest that protecting creditor rights expands firms' access to secured debt. However, the court decision was mostly relevant for firms at the vicinity of insolvency, so I hypothesize that the demand side effects should dominate in firms that are close to bankruptcy. Consistent with this hypothesis, I find that an increase in creditor protection leads to an economically large reduction in the share of secured debt for firms close to distress. The reversal of the regulation in 2004 lends further support to the demand side effects of creditor rights (Aghion et al. (1992)). Specifically, I find evidence that the 2004 court decision, by removing creditors' ability to sue managers for breach of fiduciary duty, led to a significant increase in the share of secured debt for firms close to insolvency.

I use a sample of primary issues of public bonds and private loans to investigate whether the change in creditor rights affects the characteristics of new debt issues. I begin by testing whether creditor protection rights affect the seniority of new bonds. I find that an increase in creditor rights increases the likelihood that firms issue senior, relative to subordinated, bonds by 6-13%. This probability reaches approximately

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circumstances may arise when the right (both the efficient and the fair) course to follow for the corporation may diverge from the choice that the stockholders (or the creditors, or the employees, or any single group interested in the corporation) would make if given the opportunity to act".

38% for firms that are close to bankruptcy. The reduction in the share of secured debt and the concurrent increase in senior unsecured bonds suggests that firms structure their debt to reduce creditors' ability to intervene with firm policies, but also to reduce creditor heterogeneity. Legal contests across different classes of bondholders are costly (Welch (1997)); recent litigation battles initiated by junior bondholders of Ceasars and Banco Espirito Santo in Portugal, among others, highlight how heterogeneity in creditor protection rights exacerbate and increase the costs of bankruptcy and reorganization.[2]. Along with the increase in bond seniority, I find that new bond issues are more likely to contain covenants that protect the bondholders' seniority and cross-default clauses that trigger default if the borrower becomes delinquent or violates the terms of any other contract. These findings suggest that bondholders include bond terms in the bonds that mimic the control rights of senior secured creditors.

Using a sample of private loan issues, I also find evidence that changes in creditor protection have a significant effect on loan prices and non-pricing contract terms. Banks typically have the highest seniority in firms' debt structure and usually require collateral in return for credit lines or term loans (Park (2000)). I find that the 2004 court ruling, by reversing the 1991 statute that allowed creditors to sue managers for breach of fiduciary duty, led to a 20 basis points increase in borrowers' cost of debt on average. Banks are also more likely to require collateral and substitute the loss

[2]From The Wall Street Journal (January 12th, 2015): "Bondholders Attempt to Push Caesars Unit Into Bankruptcy", and from the Financial Times (August 4th, 2014): "BES rescue saves senior bondholders"

of control rights by reducing the maturity of loan issues; the reduction in maturity further increases bank seniority over the assets of borrowers (Diamond (1993)).

This paper brings together the law and finance literature with studies that investigate the determinants of corporate debt structure. Despite the large number of studies that focus on capital structure, most empirical work treats corporate debt as uniform (Rauh and Sufi (2010)). Focusing on Delaware legal rulings to exploit variation in creditor protection rights, this study contributes to literature by providing evidence in support of the hypothesis that creditor rights explain part of the heterogeneity in firms' debt structure.

The results in this paper relate to a number of different studies. Becker and Strömberg (2012) investigate the effects of the 1991 Delaware court ruling on firms' capital structure decisions and show that improving creditor protection increases equity issuance and reduces risk taking. Ayotte and Morrison (2009) find that debt heterogeneity exacerbates inefficiencies during the bankruptcy process and suggest that the onset of senior secured creditors has an ambiguous effect on firms' debt structure. This paper is also related to Qian and Strahan (2007), who find that loan spreads are lower and the probability of issuing secured debt is higher in countries with stronger creditor protection rights. Using a natural experiment in India, Vig (2013) finds that allowing creditors to speed up the asset liquidation process leads to a reduction in the issuance of secured debt due to an increase in liquidation bias. The findings in this paper are also related to Erel et al. (2009), who find that stock market conditions affect borrowers' choice of debt.

The paper proceeds as follows. Section I describes the data and provides more details about the legal ruling in Delaware and the empirical strategy. Section II presents the results of the empirical analysis, and section III concludes.

3.2 Data and Empirical Strategy

3.2.1 Data

To test the effects of creditor protection rights on debt structure I collect three different samples. The main source for firms' accounting and debt structure information comes from Compustat's annual database. My original sample consists of 16,153 firms, 58% of which are firms incorporated in Delaware.[3] I split the sample in two periods, 1989-1995 and 2000-2007, because these periods center around significant legal rulings in Delaware that shifted managers' fiduciary duties of distressed firms towards senior secured creditors. Specifically, the first period centers around December 1991, the date that Delaware's court ruled in favor of Credit Lyonnais, a senior secured creditor, and established that the management of a firm that is close to insolvency has a fiduciary responsibility to protect creditor interests as well. The second period centers around December of 2004, the date that Delaware's court reversed 1991's decision by restricting the ability of creditors to sue corporate executives for breach of fiduciary duty.

[3]Compustat provides backfilled information for firms' state of incorporation. As a result, the actual state of incorporation of a firm in 1991 potentially may be different from the one recorded in Compustat. However, firms cannot change their state of incorporation quickly, and therefore, given the unexpected nature of the court ruling there is little concern that firms anticipate and respond strategically to the change in the legal environment (Hennessy and Strebulaev (2015)).

I collect information on firms' book value of total, secured, and senior unsecured debt. I also calculate the book value of assets and tangible assets, cash flow (ROA), market-to-book value (MB), and book leverage (BL). The average firm in the sample has approximately $834 million in assets and book value of debt at approximately 30% of its assets. To identify firms operating near bankruptcy I use Altman's Z-score (see Altman (1968)), a measure that reflects whether a firm is close to bankruptcy. I group firms into terciles of the yearly distribution of Z-score and define firms in the lowest tercile as close to bankruptcy. I also create a measure of financial distress based on a firm's market-to-book ratio similar to Davydenko and Rahaman (2011). Specifically, I define a firm as close to bankruptcy if its market value of equity is below the book value of its total debt. The correlation of the two proxies for financial distress is 44% and it is statistically significant.

I collect data on primary debt issues from two sources. First, I use Capital IQ for new issues of corporate bond debentures. The merged Compustat-Capital IQ dataset for the period 1989-1995 has 3,244 bond issues. The average corporate bond maturity in this sample is approximately 13 years, the average amount is $130 million, three quarters of new issues are senior, and the average number of bond covenants is 3.5. For each bond issue, Capital IQ provides information on its seniority, which can be Junior Subordinate, Senior Subordinate, Subordinate, Preferred, Senior Secured and Senior Unsecured. Bonds are rarely secured and less than 5% of bonds in my sample are secured. The small amount of secured bond issues is not surprising because banks commonly hold the majority of firms' secured debt. I group bonds into two broad seniority categories most commonly used in the literature. I classify a bond

issue as senior if its seniority is either Senior Secured or Senior Unsecured. In the "Junior" status I include the rest of the bonds. On average, 74% of bond issues in the sample are senior. I also count the number of bond covenants and identify their type. The first type of covenants includes provisions that protect bondholders (such as "cross-acceleration"), and the second type includes provisions that restrict borrowers' actions (such as sale of assets). Unfortunately Capital IQ does not often report bond yields and less than half of the bonds contain information on bond spreads.[4]

I also use information on private loan issues from Loan Pricing Corporation's (LPC) Dealscan database for the period 2000-2007. Dealscan contains detailed information for syndicated loan prices and loan characteristics, including financial covenants. I use the link by Chava and Roberts (2008) to match loan information with firms' accounting data from Compustat. The Compustat-Dealscan sample consists of large firms with approximately $3.1 billion in assets, which is larger than the average firm in compustat ($1.8 billion). Syndicated loans are large, approximately $300 million, and their average maturity is four years, which is considerably shorter than the average maturity of bonds. It is common for bank loans to be short-term; theory and empirical evidence suggests that creditors use collateral and maturity to protect their seniority in borrowers' debt structure (Diamond (1993), Park (2000)). The median contract has three financial covenants and there is 14.8% probability that the median firm will violate at least one of its financial covenants in any given year.

[4]Mergent FISD is another source that includes information on bond yields. However, for the period 1989-1994 the availability of bond issues in Mergent is considerably smaller than Capital IQ.

To reduce the effect of outliers I winsorize all accounting variables at the 1% level. I exclude financial firms (SIC codes 6000-6999) the financial sector is heavily regulated and a Delaware court ruling is not as relevant for these types of firms. I also exclude utilities (SIC codes 4900-4999) because the deregulation of the energy industry in the early 1990s may have confounding effects on investors' response to the court ruling.

3.2.2 Empirical Strategy

The identification strategy is based on a quasi-natural experiment that provides exogenous variation to creditor control rights. I define the treated group based on firms that are incorporated in Delaware. The control group consists of non-Delaware incorporated firms because these firms are not subject to the same legal changes. I use the following differences in differences regression model to study the impact of creditor protection rights on debt structure:

$$(\text{Secured/Total Debt})_{i,t} = \alpha_i + \alpha_t + \beta(Post1991)(Delaware) + \gamma X_{it} + \varepsilon_{it}. \quad (3.1)$$

Subscripts i and t represent the firm and year, respectively. α_i is an indicator variable for each firm and controls for the effects of unobservable firm characteristics that remain constant in time. α_t captures systematic factors in year t effecting all firms equally. X_{it} is a set of controls used to capture variation in the dependent variable. The set of controls I use in the regressions are firm size, book leverage, tangibility, return on assets (ROA), and market-to-book value.

The dependent variable *Secured/Total Debt* in regression (3.1) is the share of secured to total debt. *Post1991* is an indicator variable that takes the value of one during the period 1992-1995 and proxies for an increase in creditor protection. *Delaware* is an indicator variable that takes the value of one if the firm is incorporated in Delaware and identifies firms affected by the court ruling. The coefficient of interest is β and captures the estimated difference in the share of secured to total debt between Delaware and non-Delaware firms after the court ruling. If β is positive (negative) implies that the Delaware 1991 court ruling leads to an increase (decrease) in the share of secured to total debt. Firm and year fixed effects absorb the variation from the base effect of *Post1991* and *Delaware*, so I do not include in the regressions.

The court rulings were relevant mostly for firms closer to bankruptcy. So I hypothesize that the impact of the court ruling should be stronger for the group of firms in the zone of insolvency. To test this hypothesis I create a new variable, *Bankruptcy zone*, to identify firms that are close to distress. *Bankruptcy zone* is a dummy variable that equals one if a firm belong in the lowest tercile of Altman's Z-score yearly distribution. To test the hypothesis that the change in debt structure should be larger for firms close to distress, I use the following regression model:

$$\begin{aligned}(\text{Secured/Total Debt})_{i,t} =& \beta(Post1991)(Delaware)(Bankruptcy\ zone)+ \\ &+ \text{interaction terms} + \gamma X_{it} + \alpha_i + \alpha_t + \varepsilon_{it}\ . \qquad (3.2)\end{aligned}$$

The coefficient β of the above regression model, if negative and statistically significant, would suggest that the increase in creditor rights increases the share of secured debt

in financially distressed Delaware firms by $\beta\%$ more than non-Delaware firms. In all regressions I also include all possible combinations of the interaction terms.

Identification of causal effects in differences-in-differences regressions relies on the assumption that the dependent variable (secured/total debt) follows a similar, or parallel, trend for treated and control units before the change in Delaware's regulatory statute. Figure 1 in the Appendix confirms that the share of secured debt for Delaware and non-Delaware firms move in parallel until 1991.

Bertrand et al. (2004) find that difference in difference regressions reject the null at the 5% level approximately 45% percent of the times. I account for the potential bias in the standard errors using the appropriate variance-covariance matrix by clustering at the firm and year level. Clustering by borrowers' state of incorporation does not affect the size of the standard errors. I also address a few more of these statistical properties in the next section.

3.3 Empirical Results

3.3.1 Creditor Protection Rights and Debt Structure

This section presents the empirical results from estimating the effects of creditor control rights on corporate debt structure. I exploit variation in creditor protection rights using two Delaware court rulings; a 1991 legal ruling that established an increase in creditor protection rights, and a 2004 court ruling that reversed 1991's decision by limiting creditors' ability to sue managers for breach of fiduciary duty.

Increase in Creditor Protection Rights: Period 1989-1995

I begin by plotting the share of secured debt for a matched sample of Delaware firms (treated) and and non-Delaware firms (control). In Figure 1a, the share of secured to total debt for treated and control firms follows a similar pattern from 1988 until the passing of the regulation in 1991, and starts diverging after 1992. Assuming that in the absence of the court ruling the share of secured debt for Delaware and non-Delaware firms would maintain a parallel trend, we can infer that Delaware's court decision led to a significant reduction in the share of secured debt for firms close to distress. The results reverse after 2004, when Delaware's court decision reversed the 1991 ruling by limiting the ability of creditor to sue managers for breach of fiduciary duty. Prior to 2004 the share of secured debt for Delaware firms follows a similar trend to non-Delaware firms, but increases considerably after the 2004 court decision.

I test the effects of creditor control rights on debt structure using a large sample of firms from Compustat. Table 3.2 presents the estimates from a differences-in-differences regression model as in (3.1). The results in column 1 suggest that an increase in creditor protection does not have an effect on the share of secured debt.

However, Delaware's court shifted control rights towards creditors only for firms that are close to bankruptcy. Therefore, the court ruling should have a different effect on firms that are close to distress from well-performing firms. To test this hypothesis I use a dif-in-dif-in-dif regression model as in (3.2). To identify whether a firm is close to insolvency, I use the indicator variable *Low Z*, which identifies firms that belong at

the lowest tercile of the yearly distribution of Altman's Z-score. This regression model allows me to test whether the marginal effect of an increase in creditor protection is different for firms close to bankruptcy.

Table 3.2 shows the regression estimates of regression (3.2). I find that the increase in creditor protection rights increases the share of secured debt for firms incorporated in Delaware, but decreases considerably for firms closer to distress (coefficient next to *Post(1991)(DE)(Low Z)*. Specifically, the share of secured debt for firms closer to distress declines approximately by 4.8%, which is 1/5 of a standard deviation change of secured debt.

According to theory, there are two reasons why firms might choose to reduce the share of secured debt in response to an increase in creditor control. First, to mitigate creditors' ability to interfere with firm policy or reduce the risk of excessive liquidation (Hart et al. (1997), Strömberg (2000)). The second explanation is that firms choose to issue senior than subordinated bonds to reduce creditor heterogeneity and minimize the litigation costs from legal contests across senior and junior creditors (Welch (1997)). If this hypothesis is true, firms that are closer to bankruptcy should replace secured debt with senior unsecured bonds. The results in column (4) of Table 3.2 provide evidence consistent with this hypothesis.

However, it is also possible that the reduction in the share of secured debt is due to a reduction in firms' overall leverage. I do not find evidence consistent with the view that creditor control rights have a significant impact on leverage. Table 3.4 presents the regression results of book leverage on changes in creditor control rights.

Taken together, the results in Tables 3.2 and 3.4 suggest that the shift in managers' fiduciary duties towards creditors affects the composition of corporate debt, but not book leverage. In other words, firms replace senior secured debt with senior unsecured bonds. The next sections provide more evidence and expand the results of this section by focusing on primary issues of bonds and syndicated loans.

Reversal in Creditor Protection Rights: Period 2000-2007

Starting in 2004, Delaware's court reversed its 1991 decision by limiting lenders' ability to sue corporate executives for a breach of fiduciary responsibility.[5] This court ruling is directly related with Delaware's 1991 court decision, and offers a good empirical setting to test whether the reversal in creditor protection has the opposite effect on the share of secured debt for firms affected by the statutory changes.

Figure 1b plots the share of secured to total debt for a sample of Delaware and non-Delaware firms that are close to distress and have similar characteristics. The share of secured to total debt for Delaware and non-Delaware firms follows a parallel trend until 2004. After 2004, however, the share of secured debt for Delaware firms increases significantly.

Table 3.3 presents the results of dif-in-dif (columns 1 and 2) and dif-in-dif-dif regressions. The coefficient of *Post(2004)×(DE)×(Low Z)* is positive and economically large, consistent with the hypothesis that a reduction in creditor protection rights leads to an increase in the share of secured to total debt. However, the results are

[5]The Official Committee of Unsecured Creditors of PHD, Inc. on Behalf of the Estate of PHD, Inc. v. Bank One, N.A., et al. Case No. 03-CV-2466 (U.S. District Court, Northern District of Ohio)

statistically significant only at the 10% level, and the effect of creditor rights on senior unsecured debt does not appear to be significant.

3.3.2 Creditor Control and the Structure of Corporate Bond Seniority

In this section, I investigate the effects of the 1991 Delaware court ruling on the seniority of new bond issues. Table 3.5 presents the coefficient estimates of probit and linear probability regressions of bond seniority on the interaction of *Post1991* and *Delaware*. In column (1), the coefficient is 0.069, which suggests that after the court ruling in 1991 the probability of issuing senior debt of firms incorporated in Delaware is approximately 6.9% higher than non-Delaware firms. Using a Probit regression in column (2), I find that the increase in creditor protection leads approximately to a 13.2% increase in the probability of issuing senior relative to junior bonds. The results in Table 3.5 corroborate the findings in section 3.3.1, and suggest that the increase in creditor control rights leads to a significant increase in the likelihood that firms issue senior unsecured bonds.

The shift in managers' fiduciary duties towards creditors was relevant for firms that are closer to bankruptcy. Therefore, I hypothesize that the effect of creditor protection on bond seniority should be larger for firms that are close to distress. To test this hypothesis, I estimate a triple interaction regression model as in equation (3.2). I use two proxies for firms close to distress. *Low Z* is an indicator variable for firms that belong to the lowest tercile of the yearly distribution of Altman's Z-score, and $MVE < TD$ is an indicator variable for firms whose market value of

equity (MVE) is less than its total debt (TD).[6] The regression estimates in column (3) suggest that after the court ruling, financially distressed firms in Delaware are approximately 37% more likely to issue senior debt relatively to subordinated debt. This is an economically large effect, and robust to alternative definitions of distressed firms (column (4)).

Along with the increase in the probability of issuing senior debt, I also find that new bonds are more likely to contain covenants that protect bondholders' seniority and the collateral of the firms' assets (see also Figure 2, panel B). New bonds are also more likely to include cross-default clauses that trigger default if the borrower becomes delinquent or violates the terms of any other contract.

3.3.3 Creditor Control and Private Loan Characteristics

The law and finance literature suggests that creditor protection rights play an important role in financial development and expands firms access to debt markets by reducing their cost of borrowing (see Shleifer and Vishny (1986), Lopez-De-Silanes and La Porta (1997), La Porta et al. (1998), Levine (1998), Qian and Strahan (2007), Vig (2013)). Theory predicts that an increase in creditor protection rights should lead to a reduction in the cost of debt and a loosening of non-pricing terms such as covenants, asset collateral, or loan maturity.

To test whether changes in creditor rights affect the pricing and the characteristics of private loans, I focus on private issues of syndicated loans from Dealscan. This

[6]Note that lower values of the z-score suggest that the firm is closer to bankruptcy.

part of the analysis focuses only on the 2004 legal ruling in Delaware because loan pricing information of bond issues in Capital IQ and Dealscan before 1993 is not available.

I begin by testing the effects of creditor protection rights on firms' cost of debt. Given that the regulation is more relevant for firms closer to distress, I hypothesize that the effects of the 2004 court ruling on debt spreads should be larger for Delaware firms that are close to bankruptcy. Table 3.6 presents the estimation results of the dif-in-dif-in-dif regression of loan spreads. The results are consistent with theoretical predictions suggesting that a reduction in creditor protection should lead to an increase in the cost of debt. Specifically, I find that the cost of borrowing for Delaware firms that are close to distress increases by almost 11% more than non-Delaware firms. This an economically large effect and corresponds approximately to a 20 basis points increase in debt spreads.

I also investigate whether the reversal of creditors' ability to sue managers for breach of fiduciary duty affects non-pricing loan terms. Table 3.7 shows the estimates of the effect of a change in creditor protection on covenant strictness (column 1), collateral requirement (column 2), loan maturity (column 3), and loan amount (column 4). I find that collateral requirements increase after 2004 for Delaware firms that are close to distress. Specifically, banks are 8.2% more likely to secure new loan issues with collateral. Creditors are also more likely to substitute the loss of control by reducing the maturity of loan issues, which effectively increases their seniority over firm assets.

I find little evidence that banks change the amount of new loan issues or the strictness of loan covenants (see columns 1 and 4 of Table 3.7). However, the bar chars in Figure 3 suggest that the difference in the median covenant strictness is considerably larger after the reduction in creditor rights, even though the difference in average strictness does not change.

3.4 Conclusion

I test the hypothesis that creditor control rights affect the structure of corporate debt. Stronger creditor protection rights outside of bankruptcy increase the ability of creditors to intervene with firm policies, and as a result, firms might choose to replace secured debt for unsecured bonds. However, according to the law and finance literature, giving stronger protection rights to senior secured creditors may also increase firms' use of secured debt by reducing its cost.

Using exogenous variation from Delaware legal rulings that increase creditors control outside of bankruptcy, I find that firms closer to distress replace secured debt for unsecured bonds. Bonds are also more likely to contain covenants that protect the seniority of junior lenders and the collateral value of firm's assets. Reducing creditor protection by limiting creditors' ability to sue managers for breach of fiduciary duties affects pricing and non-pricing terms of new private loans. Specifically, weakening of creditor rights leads to an increase in the cost private loans, higher collateral requirements, and an increase in loan seniority by shortening loan maturities.

Treating debtholders as a heterogeneous class of creditors is critical in understanding the determinants of corporate debt structure. Aligning incentives among shareholders, managers, senior secured creditors, and junior unsecured bondholders is particularly difficult, especially when firms are near distress. The evidence in this paper support the hypothesis that stronger creditor protection rights explain part of firms' debt structure, and affect the seniority and the characteristics of new loan issues.

3.5 Tables & Figures Appendix

3.5.1 Tables

Definition of Variables
This table provides details for the variables used in the paper. Accounting data are from Compustat's quarterly file, bond data is from Capital IQ, and syndicated loan data is from LPC Dealscan. I winsorize all variables at the 1st and 99th percentile.

Variable Names	Description
	Firm Characteristics
Firm Size	Ln(Assets)
Book Leverage	Total Debt / Book Assets
ROA	EBITDA / Book Assets
Market-to-Book	(Market Equity + Total Debt + Preferred Stock liquidating value - Deferred Taxes and Investment Tax Credits) / Book Assets
Tangible assets	Net PPE / Book Assets

Secured/Total debt	Secured Debt (Compustat debt-mortgages and other secured) divided by Total debt (=short term+long term)
Total Debt	Short-term debt + Long-term debt
Z-Score	3.3×Pre-tax Income/Assets + 0.999×Sales/Assets + 1.4 ×Retained Earnings/Assets + 1.2 × (Current Assets - Current Liabilities)/Assets +0.6×Mkt Equity/Total Liabilities
	Loan Characteristics
Secured	Indicates whether loan is secured with collateral
Loan Spread	The All-in-Drawn Spread from Dealscan, for each dollar borrower draw, excluding fees
Covenant Number	Total number of financial covenants in the loan contract
Loan Amount	The sum of all amounts in a certain loan package
Loan Maturity	The number of months between the earliest loan origination date and the latest maturity date in a certain loan package
Covenant Strictness	Indicates the probability that the firm will violate at least one of its covenants in the next quarter. To construct the measure, I follow the methodology of Murfin (2012). To construct the measure I use covenant definitions from Demerjian and Owens (2014) and also include additional ratios often used in debt contracts

Table 3.1: **Summary Statistics**
This table presents summary statistics for three samples. The top panel shows firm characteristics for all firms in Compustat from 1989-2007. The middle panel shows bond characteristics for all corporate debentures from Capital IQ during the period 1989-1995. The bottom panels shows the characteristics from all syndicated loans in Dealscan during the period 2000-2007. All variables are described in detail in the Appendix.

Variable	**N**	**Mean**	**SD**	**P10**	**Median**	**P90**
	Compustat (Full Period)					
Assets - Total	152728	834.59	2696.52	2.35	66.26	1663.18
Mkt-to-Book	129747	3.09	7.10	0.58	1.25	5.12
B.Leverage	151384	0.29	0.34	0.00	0.20	0.69
Tangible Assets	151710	0.27	0.24	0.03	0.19	0.66
Altman-Z	124948	2.85	14.66	-2.09	3.05	11.58
Secured/Total Debt	127025	0.35	0.37	0.00	0.19	0.96
	Bond Variables (Period: 1989-1995)					
Maturity (months)	3244	161.375	103.857	61	122	365
Amount (m.$)	3243	128.728	191.522	2	100	300
Senior	3244	0.745	0.436	0	1	1
Number of Covenants	3244	3.568	4.077	0	3	10
	Syndicated Loan Variables (Period: 2000-2007)					
Cov. Strictness	9253	25.19	27.07	0.00	14.81	66.54
Covenants #	9340	2.83	0.95	1.00	3.00	4.00
Loan Amount	9340	297.41	455.88	14.07	135.16	750.00
Loan Maturity	9260	44.23	20.76	12.00	45.11	66.00
Spread	9006	199.24	132.22	50.00	175.00	360.00
Secured	8335	0.72	0.45	0.00	1.00	1.00

Table 3.2: **Increase in creditor protection rights and debt structure: 1991 Delaware legal ruling (Period 1989-1995)**.
This table presents coefficient estimates of fixed effect regressions that examine the effect of an increase in creditor protection on the share of secured to total debt (columns 1 and 2) and senior unsecured debt to total debt (columns 3 and 4). *DE* is an indicator variable that takes the value of 1 if the state of incorporation of the issuing firm is Delaware. *Post 1991* is an indicator variable that takes the value of 1 during the period 1992-1995. In columns 2 and 4, *Low Z* is an indicator variable that is equal to 1 if the firm belongs to the bottom tercile of the yearly distribution of Altman's Z-score. All regressions include firm and year fixed effects. I also include a set of firm controls: size (*Ln(Assets)*), tangible assets, ROA, book leverage, and market-to-book value. All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) (Secured/Tot)	(2) (Secured/Tot)	(3) (Senior/Tot)	(4) (Senior/Tot)
Delaware Rulings				
Post(1991)×(DE)	0.003	0.025***	-0.013	-0.028***
	(0.005)	(0.007)	(0.008)	(0.007)
Post(1991)×(DE)×(Low Z)		-0.048**		0.039*
		(0.015)		(0.020)
Controls				
Ln(Assets)	-0.008**	-0.008**	0.011**	0.0012**
	(0.004)	(0.004)	(0.005)	(0.006)
Tangible Assets	0.087***	0.089***	-0.114***	-0.114**
	(0.032)	(0.029)	(0.033)	(0.034)
ROA	0.034**	0.036*	-0.050**	-0.046**
	(0.017)	(0.019)	(0.016)	(0.016)
B.Leverage	-0.045**	-0.050**	-0.031	-0.036
	(0.022)	(0.024)	(0.029)	(0.031)
Mkt-to-Book	0.000	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	18244	17297	22730	21654
AR^2	0.080	0.081	0.387	0.391
Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Table 3.3: **Decrease in creditor protection rights and debt structure: 2004 Delaware legal ruling (Period 2000-2007).**
This table presents coefficient estimates of fixed effect regressions that examine the effect of enhanced creditor rights on the share of secured to total debt (columns 1 and 2) and senior unsecured debt to total debt (columns 3 and 4). *DE* is an indicator variable that takes the value of 1 if the state of incorporation of the issuing firm is Delaware. *Post 2004* is an indicator variable that takes the value of 1 during the period 2005-2007. In columns 2 and 4, *Low Z* is an indicator variable that is equal to 1 if the firm belongs to the bottom tercile of the yearly distribution of Altman's Z-score. All regressions include firm and year fixed effects. I also include a set of firm controls: size (*Ln(Assets)*, tangible assets, ROA, book leverage, and market-to-book value. All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) (Secured/Tot)	(2) (Secured/Tot)	(3) (Senior/Tot)	(4) (Senior/Tot)
Delaware Rulings				
Post(2004)×(DE)	0.016**	0.004***	-0.032	0.021
	(0.005)	(0.001)	(0.022)	(0.042)
Post(2004)×(DE)×(Low Z)		0.025*		-0.100
		(0.013)		(0.107)
Controls				
Ln(Assets)	-0.012***	-0.013**	0.031**	0.036**
	(0.004)	(0.006)	(0.015)	(0.015)
Tangible Assets	0.059*	0.069**	-0.232**	-0.249**
	(0.034)	(0.033)	(0.996)	(0.341)
ROA	0.013*	0.011*	-0.002	-0.003
	(0.007)	(0.006)	(0.020)	(0.018)
B.Leverage	0.017***	0.016***	-0.044***	-0.038**
	(0.005)	(0.005)	(0.016)	(0.019)
Mkt-to-Book	0.001	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Observations	17115	16589	18835	18261
AR^2	0.075	0.076	0.411	0.411
Firm FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

Table 3.4: **Creditor protection rights and book leverage**
This table presents coefficient estimates of fixed effect regressions that examine the effect of creditor rights on book leverage (columns 1 and 2). The dependent variable in all regressions is book leverage. *DE* is an indicator variable that takes the value of 1 if the state of incorporation of the issuing firm is Delaware. *Post 2004* is an indicator variable that takes the value of 1 during the period 2005-2007. In columns 2 and 4, *Low Z* is an indicator variable that is equal to 1 if the firm belongs to the bottom tercile of the yearly distribution of Altman's Z-score. All regressions include firm and year fixed effects. The regressions control for a set of firm characteristics (assets, tangible assets, ROA, book leverage, market-to-book value) and loan characteristics (maturity, amount, participants, collateral, and spread). All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(*1991: Creditor Rights* ↑)	(*2004: Creditor Rights* ↓)
	(Leverage)	(Leverage)
Delaware Rulings		
Post(1991)×(DE)×(Low Z)	-0.004	
	(0.006)	
Post(2004)×(DE)×(Low Z)		-0.005
		(0.032)
Controls		
Post(1991)×(DE)	0.005**	
	(0.002)	
Post(2004)×(DE)		0.007
		(0.007)
Ln(Assets)	0.027***	0.021***
	(0.002)	(0.004)
Mkt-to-Book	-0.000	-0.001
	(0.001)	(0.001)
Tangible Assets	0.141***	0.172***
	(0.014)	(0.038)
ROA	-0.115***	-0.079***
	(0.017)	(0.012)
Observations	40777	24695
AR^2	0.196	0.153
Firm FEs	Yes	Yes
Year FEs	Yes	Yes

Table 3.5: **Increase in creditor protection rights (1991) and bond seniority: (Period 1989-1995)**
DE is an indicator variable that takes the value of 1 if the state of incorporation of the issuing firm is Delaware. *Post 1991* is an indicator variable that takes the value of 1 during the period 1992-1995. *Low Z* is an indicator variable that is equal to 1 if the firm belongs to the bottom tercile of the yearly distribution of Altman's Z-score. *MVE<TD* is an indicator variable that takes the value of 1 if the firm's market value of equity is less than its total debt. All regressions include firm and year fixed effects. I also include a set of firm controls: size (*Ln(Assets)*), tangible assets, ROA, book leverage, and market-to-book value. All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

Variables	(1) (Senior Bond) (*OLS with FEs*)	(2) (Senior Bond) (*Probit*)	(3) (Senior Bond) (*OLS with FEs*) (Low Z)	(4) (Senior Bond) (*OLS with FEs*) (MVE<TD)
(DE)×(Post 1991)	0.069**	0.132***	0.039	0.046
	(0.029)	(0.035)	(0.031)	(0.029)
(Post 1991)×(DE)×(Low Z)			0.378**	
			(0.179)	
(Post 1991)×(DE)×(MVE<TD)				0.261*
				(0.141)
Observations	2345	2345	2345	1831
(Pseudo)R^2	0.838	0.430	0.840	0.842
Firm FE	Yes	No	Yes	Yes
Year FE	Yes	No	Yes	Yes

Table 3.6: **Decrease in creditor protection rights (2004) and cost of debt (Period 2000-2007).**
This table presents coefficient estimates of fixed effect regressions that examine the effect of enhanced creditor rights on the cost of debt. *DE* is an indicator variable that takes the value of 1 if the state of incorporation of the issuing firm is Delaware. *Post 2004* is an indicator variable that takes the value of 1 during the period 2005-2007. In columns 2 and 4, *Low Z* is an indicator variable that is equal to 1 if the firm belongs to the bottom tercile of the yearly distribution of Altman's Z-score. All regressions include firm and year fixed effects. The regressions control for a set of firm characteristics (*Ln(Assets)*, tangible assets, ROA, book leverage, market-to-book value) and loan characteristics (maturity, amount, participants, collateral, and spread). All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Ln(Spread)
Delaware Rulings	
Post(2004)×(DE)×(Low Z)	0.114**
	(0.045)
Base Effects and Interactions	
Post(2004)×(DE)	-0.027
	(0.049)
(DE)×(Low Z)	-0.001
	(0.049)
Post(2004)×(Low Z)	-0.156**
	(0.066)
(Low Z)	0.210***
	(0.045)
(DE)	0.040
	(0.051)
Observations	5019
AR^2	0.668
SIC-3 FEs	Yes
Year FEs	Yes

Table 3.7: **Decrease in creditor protection rights and loan contracting (Period 2000-2007).**
In column (1) the dependent variable is covenant strictness following the method from Murfin (2012) and measures the probability that the firm will violate at least one of performance covenants in the next quarter. In column (2) the dependent variable is an indicator variable that takes the value of 1 if the bank required to secure the loan issue with collateral. In columns (3) and (4), the dependent variable is the natural log of loan maturity and loan amount, respectively. *DE* is an indicator variable that takes the value of 1 if the state of incorporation of the issuing firm is Delaware. *Post 2004* is an indicator variable that takes the value of 1 during the period 2005-2007. In columns 2 and 4, *Low Z* is an indicator variable that is equal to 1 if the firm belongs to the bottom tercile of the yearly distribution of Altman's Z-score. All regressions include industry (SIC-3) and year fixed effects. The regressions control for a set of firm and loan characteristics. All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and year level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Cov.Strictness	(2) Secured	(3) Ln(Maturity)	(4) Ln(Amount)
Delaware Rulings				
Post(2004)×(DE)×(Low Z)	3.199	0.082**	-0.123*	-0.067
	(4.170)	(0.030)	(0.063)	(0.096)
Base Effects and Interactions				
Post(2004)×(DE)	1.128	-0.045*	-0.046**	0.117*
	(2.460)	(0.019)	(0.017)	(0.051)
(DE)×(Low Z)	-1.081	0.004	0.080*	0.089
	(3.730)	(0.030)	(0.037)	(0.060)
Post(2004)×(Low Z)	2.333	-0.005	0.150**	0.011
	(2.584)	(0.033)	(0.056)	(0.069)
(Low Z)	2.287	0.090***	-0.227***	-0.333***
	(1.930)	(0.022)	(0.026)	(0.055)
(DE)	-3.045	0.050**	0.042	-0.116**
	(2.344)	(0.019)	(0.025)	(0.040)
Observations	5020	5211	5530	5748
AR^2	0.282	0.409	0.246	0.807
SIC-3 FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes

3.5.2 Figures

Figure 3.1: **Share of secured debt (period: 1989-1995)**
This figure illustrates the share of secured debt during the 1999-2007 period. I match firms with similar characteristics in Delaware (DE) and non-Delaware (non-DE) that belong in the bottom tercile of Altman's Z-score yearly distribution.

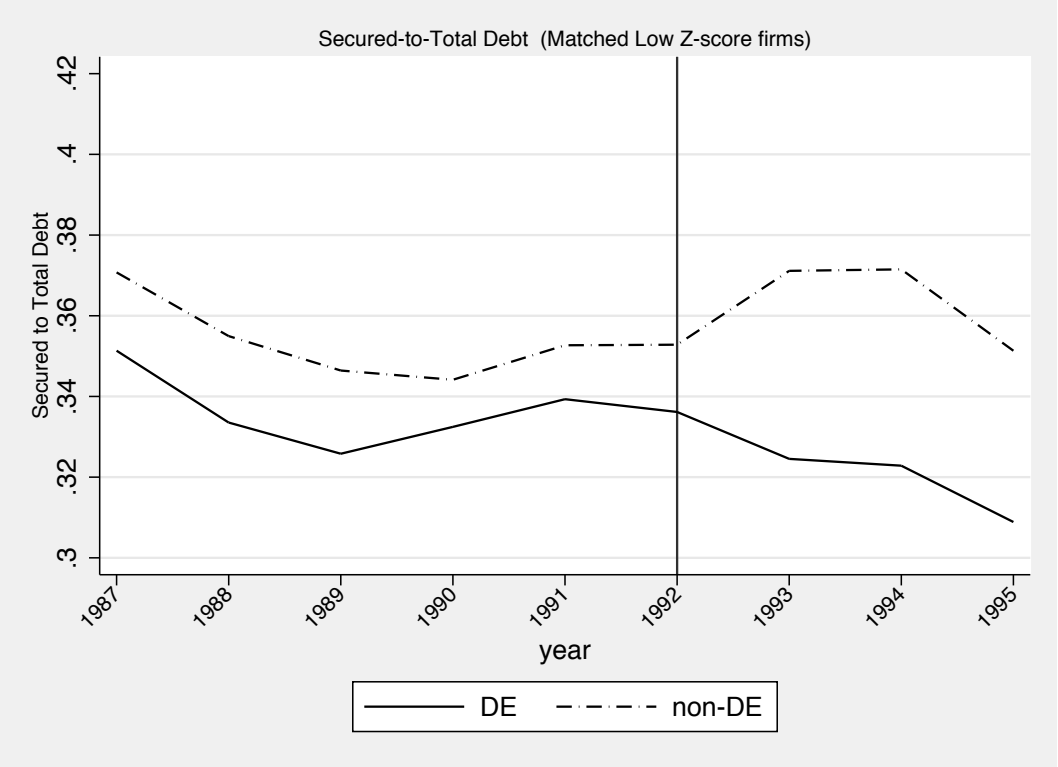


Figure 3.2: **Share of secured debt (period: 2000-2007)**
This figure illustrates the share of secured debt during the 1999-2007 period. I match firms with similar characteristics in Delaware (DE) and non-Delaware (non-DE) that belong in the bottom tercile of Altman's Z-score yearly distribution.

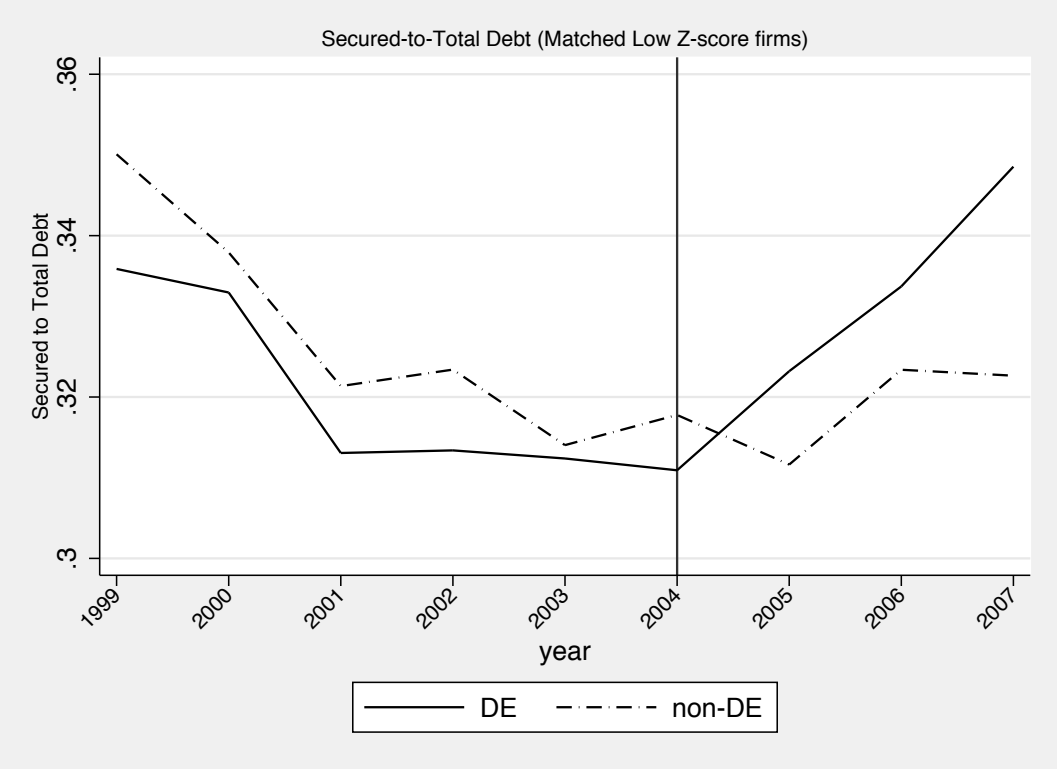


Figure 3.3: **Bond seniority and covenants**
The top panel illustrates the issuance of senior debt from Delaware firms (treated group) and non-Delaware firms (control group) from 1989-1995. The panel at the bottom illustrates the difference in the number of restrictive covenants between Delaware and non-Delaware firms (absolute number) from 1989-1995.

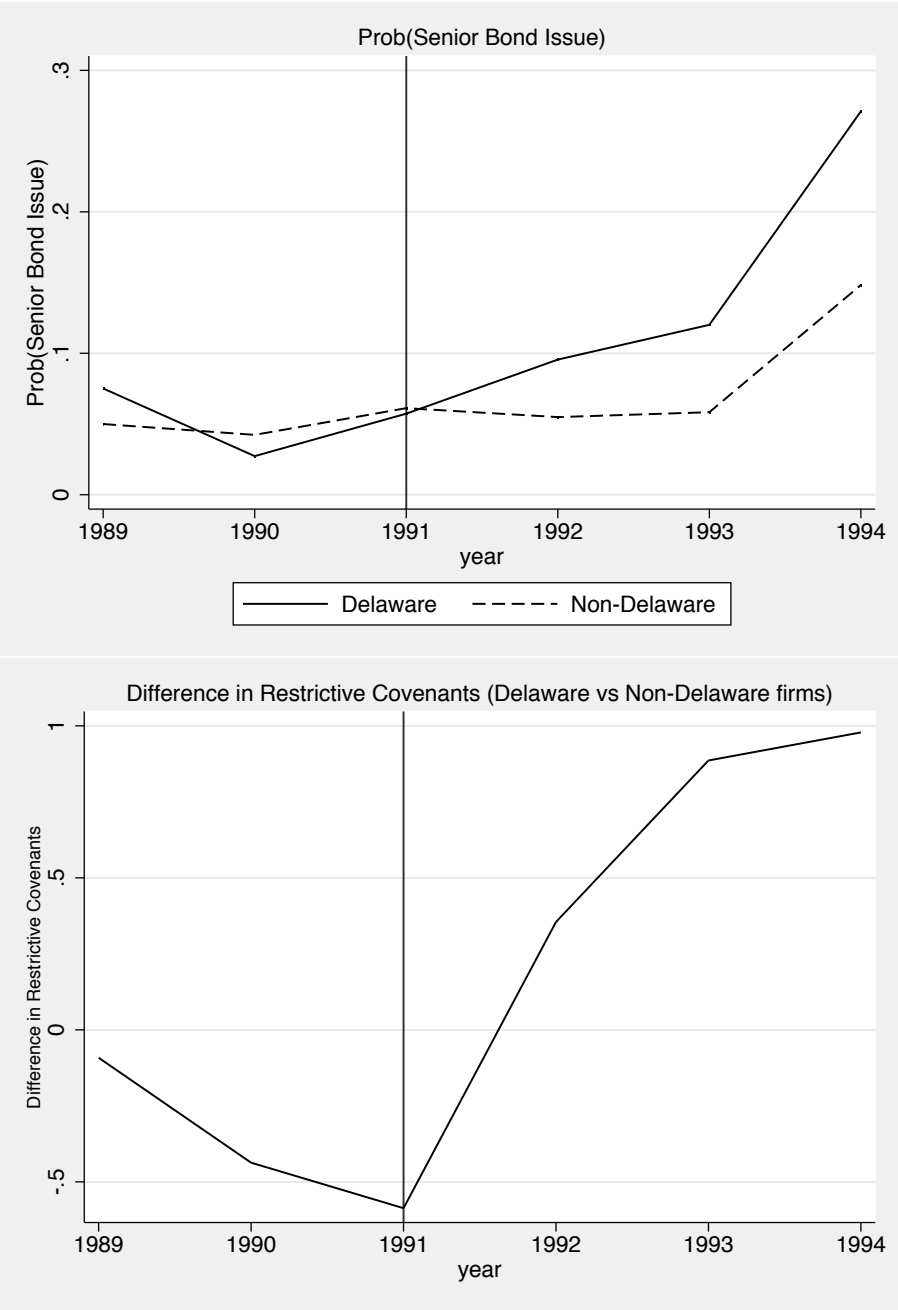
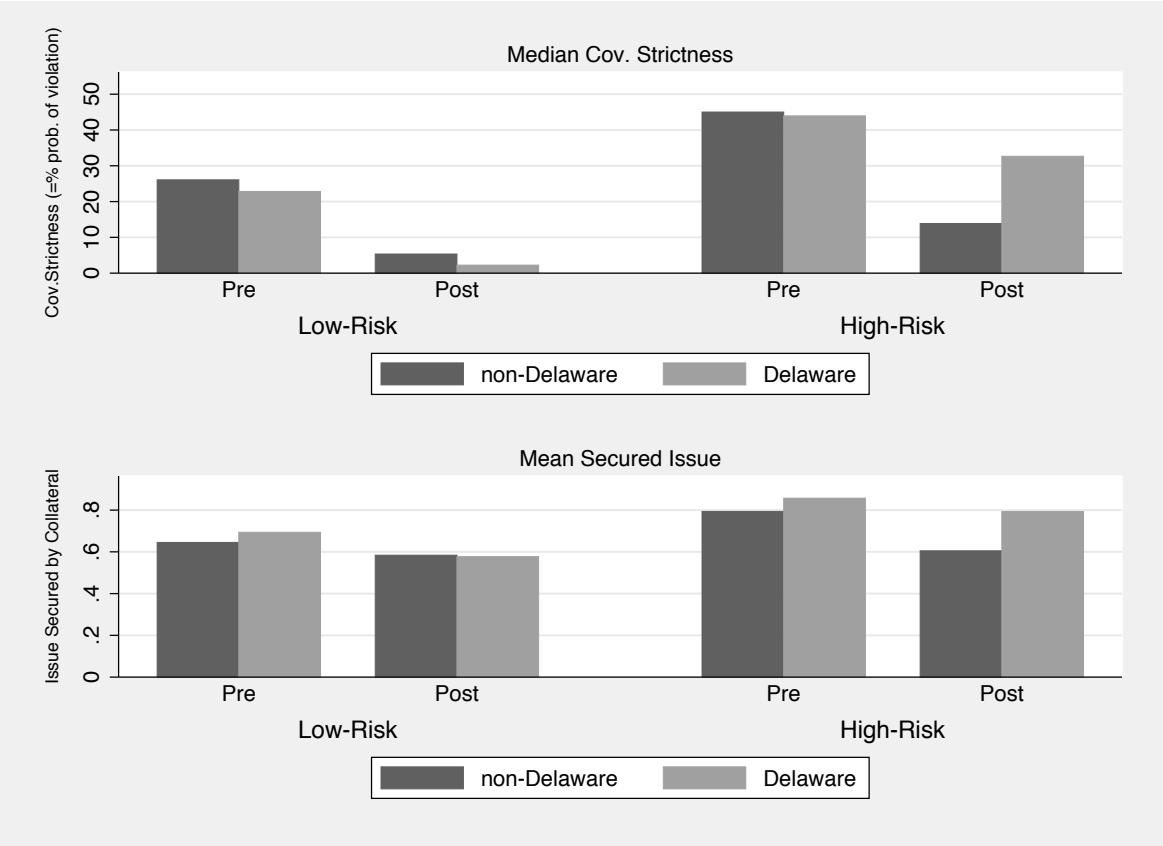


Figure 3.4: Median covenant strictness (top panel) and average covenant strictness (bottom panel) for Delaware firms and non-Delaware firms.



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